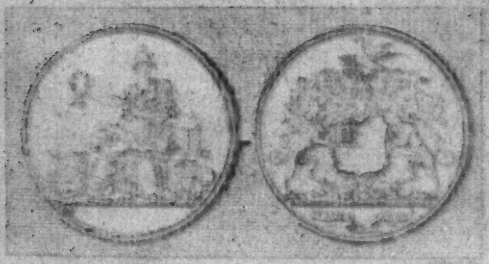
PHILOSOPHICAL TRANSACTIONS.

PART II.



PHILOSOPHICAL

TRANSACTIONS

PART II.

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OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXIX. For the Year 1779.

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LONDON,

PRINTED BY J. NICHOLS, SUCCESSOR TO MR. BOWYER;

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MDCCLXXX.

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CONTENTS

OF

VOL. LXIX. PART II.

XXII. *AN ACCOUNT of the Manner in which the* Ruſſians *treat Perſons affected by the Fumes of burning Charcoal, and other Effluvia of the ſame Nature. In a Letter from* Matthew Guthrie, *M. D. to* Joſeph Prieſtley, *LL.D. F. R. S.* page 325

XXIII. *An Account of an Apparatus applied to the equatorial Inſtrument for correcting the Errors ariſing from the Refraction in Altitude. By Mr.* Peter Dollond, *Optician; communicated by the Aſtronomer Royal.* p. 332

XXIV. *Experiments and Obſervations on the inflammable Air breathed by various Animals. By the Abbé* Fontana, *Director of the Cabinet of Natural Hiſtory belonging to his Royal Highneſs the Grand Duke of* Tuſcany; *communicated by* John Paradiſe, *Eſq. F. R. S.* p. 337

XXV. *On the Variation of the Temperature of boiling Water. By Sir* George Shuckburgh, *Baronet, F. R.* and

and A. S. and Member of the Academy of Sciences and Belles Lettres at Lyon. p. 362

XXVI. *Account of a new Kind of inflammable Air or Gass, which can be made in a Moment without Apparatus, and is as fit for Explosion as other inflammable Gasses in use for that Purpose; together with a new Theory of Gunpowder. By* John Ingen Housz, *Body Physician to their Imperial Majesties, and F. R. S.* p. 376

XXVII. *The Description of Two new Micrometers. By Mr.* Ramsden, *Optician; communicated by* Joseph Banks, *Esq. P. R. S.* p. 409

XXVIII. *Account of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. In a Letter from the Abbé* Fontana, *Director of the Cabinet of Natural History belonging to his Royal Highness the Grand Duke of* Tuscany, *to* Joseph Priestley, *LL. D. F. R. S.* p. 432

XXIX. *Account of some Experiments in Electricity. In a Leter from Mr.* William Swift *to the Rev. Dr.* King, *F. R. S.* p. 454

XXX. Sitodium *incisum et macrocarpon, ususque fructuum qui exinde nascuntur, descripta a* Carolo Petro Thunberg, *M. D.; communicated by* Joseph Banks, *Esq. P. R. S.* p. 462

XXXI. *A Second Paper concerning some Barometrical Measures in the Mines of the* Hartz. *By Mr.* John Andrew De Luc, *F. R. S.* p. 485

XXXII. *On the Precession of the Equinoxes produced by the Sun's Attraction. By the Rev. Mr.* Isaac Milner, *M. A.*

M. A. and Fellow of Queen's College, Cambridge; *communicated by the Rev. Dr.* Shepherd, *F. R. S.* p. 505

XXXIII. *An Examination of various Ores in the Muſeum of Dr.* William Hunter. *By* George Fordyce, *M. D. F. R. S. and Mr.* Staneſby Alchorne. p. 527

XXXIV. *On ſome new Methods of ſuſpending Magnetical Needles. By* John Ingen Houſz, *Body Phyſician to their Imperial Majeſties, and F. R. S.* p. 537

XXXV. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1778. *By* Thomas Barker, *Eſquire. Communicated by* Thomas White, *Eſquire, F. R. S.* p. 547

XXXVI. *Extract of a Meteorological Journal for the Year* 1778, *kept at* Briſtol, *by* Samuel Farr, *M. D.* p. 551

XXXVII. *A Treatiſe on Rivers and Canals. By* Theod. Aug. Mann, *Member of the Imperial and Royal Academy of Sciences at* Bruſſels; *communicated by* Joſeph Banks, *Eſq. P. R. S.* p. 555

XXXVIII. *Extract of Two Meteorological Journals of the Journals of the Weather, obſerved at* Nain, in 57° *North latitude, and at* Okak, *in* 57° 30′ *North latitude, both on the Coaſt of* Labradore. *Communicated by Mr.* De la Trobe *to the Preſident, and by him to the Society.* p. 657

XXXIX. *Improvements in Electricity. By* John Ingen Houſz, *Body Phyſician to their Imperial Majeſties, and F. R. S.; who was nominated by the Preſident and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late* Henry Baker, *Eſq. F. R. S.* p. 659

PHILOSOPHICAL TRANSACTIONS.

XXII. *Account of the Manner in which the* Ruſſians *treat Perſons affected by the Fumes of burning Charcoal, and other Effluvia of the ſame Nature. In a Letter from* Matthew Guthrie, *M. D. to* Joſeph Prieſtley, *LL.D. F. R. S.*

Read March 4, 1779.

DEAR SIR, St. Peterſburg, October 12, 1778.

I SHALL endeavour to recollect, according to your deſire, the particulars of that part of my former letter which related to the mode of recovering people in Ruſſia, who are apparently deprived of life by the prin-

ciple emitted from burning charcoal, or by the incruſtation formed upon the inſides of the boors huts when it thaws.

People of condition in this country have double windows to their houſes in winter; but the commoner ſort have only ſingle ones, which is the reaſon that, during a ſevere froſt, there is an incruſtation formed upon the inſides of the glaſs windows. This ſeems to be compoſed of condenſed breath, perſpiration, &c. as a number of people live and ſleep in the ſame ſmall room, eſpecially in great cities. This excrementitious cruſt is farther impregnated with the phlogiſton of candles, and of the oven with which the chamber is heated.

When a thaw ſucceeds a hard froſt of long duration, and this plate of ice is converted into water, there is a principle ſet looſe, which produces all the terrible effects upon the human body which the principle emitted from charcoal is ſo well known to do in this country, where people every day ſuffer from it. However, the Ruſſians conſtantly lay the blame upon the oven, when they are affected by the thawing of the cruſt, as the effects are perfectly ſimilar, and they cannot bring themſelves to believe, that the diſſolving of ſo ſmall a portion of ice can be attended with any bad conſequence, when they daily melt larger maſſes without danger: yet the oven

does

does not at all account for the complaints brought on at this period; for, upon examination, they generally find every thing right there, and ſtill the *ugar*, or hurtful vapour, remaining in the room.

As the effects of both are ſimilar, as I have ſaid above, and likewiſe the mode of recovery, I ſhall only give you an account of the operation of the principle emitted by burning charcoal, and of the method of bringing thoſe people to life who have been *ſuffocated* by it (as I think it is erroneouſly termed); this will ſuperſede the neceſſity of giving the hiſtory of both, or rather it will be giving both at the ſame time.

Ruſſian houſes are heated by the means of ovens; and the manner of heating them is as follows. A number of billets of wood are placed in the peech or ſtove, and allowed to burn till they fall in a maſs of bright red cinders; then the vent above is ſhut up, and likewiſe the door of the peech which opens into the room, in order to concentrate the heat; this makes the tiles of which the peech is compoſed as hot as you deſire, and ſufficiently warms the apartment; but ſometimes a ſervant is ſo negligent as to ſhut up the peech or oven before the wood is ſufficiently burnt, for the red cinders ſhould be turned over from time to time to ſee that no bit of wood remains of a blackiſh colour, but that the whole maſs is

of a uniform glare (as if almoſt tranſparent) before the openings are ſhut, elſe the *ugar* or vapour is ſure to ſucceed to miſmanagement of this ſort, and its effects are as follows.

If a perſon lays himſelf down to ſleep in the room expoſed to the influence of this vapour, he falls into ſo ſound a ſleep that it is difficult to awake him, but he feels (or is ſenſible of) nothing. There is no ſpaſm excited in the *trachea arteria* or lungs to rouſe him, nor does the breathing, by all accounts, ſeem to be particularly affected: in ſhort, there is no one ſymptom of ſuffocation; but towards the end of the cataſtrophe, a ſort of groaning is heard by people in the next room, which brings them ſometimes to the relief of the ſufferer. If a perſon only ſits down in the room, without intention to ſleep, he is, after ſome time, ſeized with a drowzineſs and inclination to vomit. However, this laſt ſymptom ſeldom affects a Ruſſian, it is chiefly foreigners who are awaked to their dangers by a *nauſea*; but the natives, in common with ſtrangers, perceive a dull pain in their heads, and if they do not remove directly, which they are often too ſleepy to do, are ſoon deprived of their ſenſes and power of motion, inſomuch, that if no perſon fortunately diſcovers them within an hour after this worſt ſtage, they are irrecoverably loſt; for the Ruſſians ſay, that they

do not ſucceed in reſtoring to life thoſe who have lain more than an hour in a ſtate of inſenſibility.

The recovery is always attempted, and often effected, in this manner. They carry the patient immediately out of doors, and lay him upon the ſnow, with nothing on him but a ſhirt and linen drawers. His ſtomach and temples are then well rubbed with ſnow, and cold water, or milk is poured down his throat. This friction is continued with freſh ſnow until the livid hue, which the body had when brought out, is changed to its natural colour, and life renewed; then they cure the violent head-ach which remains by binding on the forehead a cataplaſm of black rye bread, and vinegar.

In this manner the unfortunate man is perfectly reſtored, without blowing up the lungs, as is neceſſary in the caſe of drowned perſons; on the contrary, they begin to play of themſelves ſo ſoon as the ſurcharge of phlogiſton makes its eſcape from the body.

It is well worthy of obſervation, how diametrically oppoſite the modes are of reſtoring to life, thoſe who are deprived of it by water, and thoſe who have loſt it by the fumes of charcoal: the one conſiſting in the internal and external application of heat, and the other in that of cold. It may be alledged, that the ſtimulus of the cold produces heat, and the fact ſeems to be confirmed by the Ruſſian method of reſtoring circulation in a frozen limb.

limb by means of friction with ſnow. But what is ſingular in the caſe of people apparently deprived of life in the manner treated of is, that the body is much warmer when brought out of the room than at the inſtant life is reſtored, and that they awake cold and ſhivering. The colour of the body is alſo changed from a livid red to its natural complexion, which, together with ſome other circumſtances, would almoſt lead one to ſuſpect, that they are reſtored to life by the ſnow and cold water ſome how or other freeing them from the load of phlogiſton with which the ſyſtem ſeems to be replete; for although the firſt application of cold water to the human body produces heat, yet, if often repeated in a very cold atmoſphere, it then cools inſtead of continuing to heat, juſt as the cold bath does when a perſon remains too long in it.

In ſhort, I think it is altogether a curious ſubject, whether you take into conſideration the mode of action of the principle emitted by burning charcoal, and our phlogiſticated cruſt; or the operation of the ſnow and cold water. However, I ſhall by no means take upon me to decide, whether the dangerous ſymptoms related above are produced by the air in the room being ſo ſaturated with phlogiſton as to be unable to take up the proper quantity from the lungs, which occaſions a ſurcharge in the ſyſtem, according to your theory, or whe-

ther ſo ſubtle a fluid may ſomehow find its way into the circulation, and thereby arreſt the vital powers; nor ſhall I determine whether the livid hue of the body when brought out is changed into a paler colour by the atmoſphere ſomehow or other abſorbing and freeing the blood from the colouring principle, as you have ſhewn to be the caſe with blood out of the body: theſe are curious inquiries that I ſhall leave to your inveſtigation. I have only endeavoured to collect facts from a number of natives who have met with this accident themſelves, or have aſſiſted in reſtoring others to life. It is ſo common a caſe here that it is perfectly familiar to them, and they never call in medical aſſiſtance.

I am, &c.

XXIII. *An Account of an Apparatus applied to the equatorial Inſtrument for correcting the Errors ariſing from the Refraction in Altitude.* By Mr. Peter Dollond, *Optician; communicated by the Aſtronomer Royal.*

Read March 4, 1779.

THE refraction of the atmoſphere occaſions the ſtars or planets to appear higher above the horizon than they really are; therefore, a correction for this refraction ſhould be made in a vertical direction to the horizon.

The equatorial inſtrument is ſo conſtructed, that the correction cannot be made by the arches or circles which compoſe it when the ſtar, &c. is in any other vertical arch except that of the meridian, becauſe the declination arch is never in a vertical poſition but when the teleſcope is in the plane of the meridian.

To correct this error, a method of moving the eye-tube which contains the wires of the teleſcope in a vertical direction to the horizon has been practiſed; but as the eye-tube is obliged to be turned round in order to move

move it in that direction, in the different oblique positions of the inſtrument, the wires are thereby put out of their proper ſituation in every other poſition of the inſtrument, except when it is in the plane of the meridian; for the equatorial wire ſhould always be parallel to the equator, that the ſtar in paſſing over the field of the teleſcope may move along with it, otherwiſe one cannot judge whether the teleſcope be ſet to the proper declination, except at the inſtant the ſtar is brought to the interſection of the wires, which is only a momentary obſervation.

The method I have now put in practice for correcting the refraction of the atmoſphere is by applying two lenſes before the object-glaſs of the teleſcope; one of them convex, and the other concave; both ground on ſpheres of the ſame radius, which in thoſe I have made is thirty feet. The convex lens is round, of the ſame diameter as the object-glaſs of the teleſcope, and fixed into a braſs frame or apparatus, which fits on to the end of the teleſcope. The concave lens is of the ſame width, but nearly two inches longer than it is wide, and is fixed in an oblong frame, which is made to ſlide on the frame the other lens is fixed into, and cloſe to it. Theſe two lenſes being wrought on ſpheres of the ſame radius, the refraction of the one will be exactly deſtroyed by that of

the other, and the focal length of the object-glaſs will not be altered by their being applied before it; and if the centers of theſe two lenſes coincide with each other, and alſo with that of the object-glaſs, the image of any object formed in the teleſcope will not be moved or ſuffer any change in its poſition. But if one of the lenſes be moved on the other, in the direction of a vertical arch, ſo as to ſeparate its center from that of the other lens, it will occaſion a refraction, and the image will change its altitude in the teleſcope. The quantity of the refraction will be always in proportion to the motion of the lens, ſo that by a ſcale of equal parts applied to the braſs frame, the lens may be ſet to occaſion a refraction equal to the refraction of the atmoſphere in any altitude. If the concave lens be moved downwards, that is, towards the horizon, its refraction will then be in a contrary direction to that of the atmoſphere, and the ſtar will appear in the teleſcope as if no refraction had taken place.

There is a ſmall circular ſpirit level fixed on one ſide the apparatus, which ſerves to ſet it in ſuch a poſition, that the centers of the two lenſes may be in the plane of a vertical arch. This level is alſo uſed for adjuſting a ſmall quadrant, which is fixed to it, and divided into degrees, to ſhew the elevation of the teleſcope when directed to the ſtar; then the quantity of refraction anſwering

ſwering to that altitude may be found by the common tables, and the concave lens ſet accordingly, by means of the ſcale at the ſide, which is divided into half minutes, and, if required, by uſing a nonius, may be divided into ſeconds.

It muſt be obſerved, that when a ſtar or planet is but a few degrees above the horizon, the refraction of the atmoſphere occaſions it to be conſiderably coloured. The refraction of the lens acting in a contrary direction would exactly correct that colour, if the diſſipation of the rays of light were the ſame in glaſs as in air; but as it is greater in glaſs than in air, the colours occaſioned by the refraction of the atmoſphere will be rather more than corrected by thoſe occaſioned by the refraction of the lens.

A drawing of the refraction apparatus is added, which may ſerve to give a more clear idea of it. See plate IV.

EXPLANATION OF THE PLATE.

AA, The circular braſs tube, which fits on to the end of the teleſcope.

BB, The oblong concave lens in its frame, which ſlides over the fixed convex lens.

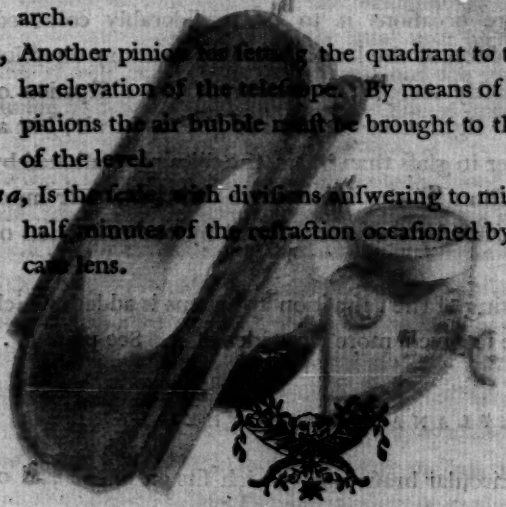
c, The circular ſpirit level, which ſhews when the oblong lens is in a vertical arch.

D, The quadrant to which the ſpirit level is fixed, for ſhewing the angular elevation of the teleſcope.

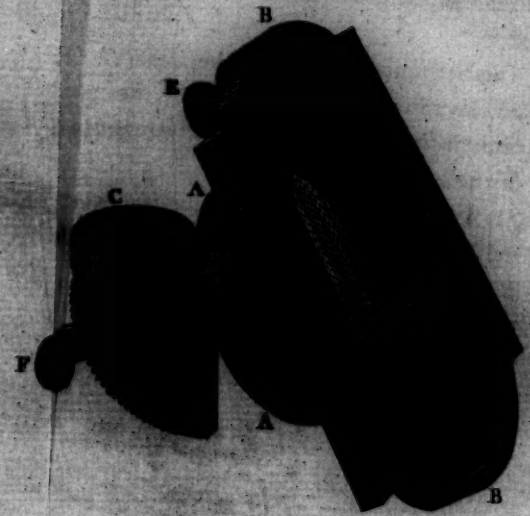
E, The milled head fixed to a pinion, by which the whole apparatus is turned round on the end of the teleſcope, in order to ſet the oblong lens in a vertical arch.

F, Another pinion for fetting the quadrant to the angular elevation of the teleſcope. By means of theſe two pinions the air bubble muſt be brought to the middle of the level.

a a, Is the ſcale, with diviſions anſwering to minutes and half minutes of the refraction occaſioned by the concave lens.



Philos. Trans. Vol. LXIX. Tab. IV. p. 356.



XXIV. *Experiments and Obſervations on the inflammable Air breathed by various Animals. By the Abbé* Fontana, *Director of the Cabinet of Natural Hiſtory belonging to his Royal Highneſs the Grand Duke of* Tuſcany; *communicated by* John Paradiſe, *Eſq. F. R. S.*

Read March 11, 1779.

PHILOSOPHERS believed, till lately, that inflammable air had the power of killing animals who breathed it. Dr. PRIESTLEY, to whom we are much indebted for many diſcoveries and obſervations relative to inflammable air, made in conſequence of Mr. CAVENDISH's excellent paper on that ſubject, aſſures us, that inflammable air cauſes the death of animals as readily as fixed air, and that animals die convulſed in it. The doctor adds, that water abſorbed about one quarter of the inflammable air ſhaken in it, after which a mouſe lived in it as long as it would have lived in an equal quantity of common air. This air breathed by the mouſe was ſtill inflammable, though not ſo much as before.

Mr.

Mr. SHEELE, who has made various important obſervations in chemiſtry, on the contrary aſſerts, that inflammable air not only does not kill the animals who breathe it, but that it is even good and innocent air. He relates ſome experiments to which it ſeems that nothing can be oppoſed, and they appear to contradict Dr. PRIESTLEY's obſervations. Mr. SHEELE has breathed inflammable air contained in a bladder, without receiving any hurt.

Seeing then that the experiments of theſe celebrated perſons contradicted each other, I began to ſuſpect that they might poſſibly be all true; and that their ſo contradictory effects might be owing to ſome circumſtance not yet attended to.

In order to follow ſome method in my reſearches about a point ſo delicate, and which ſo nearly intereſts human life, I firſt of all thought of aſſuring myſelf, whether or no animals could breathe inflammable air with impunity, when the receivers that contained it were immerſed in quickſilver. To this end, I introduced inflammable air, extracted both from zinc and iron, by means of the vitriolic acid, into various tubes filled with quickſilver, in which the air entered pretty free from moiſture. I then introduced various birds into thoſe tubes, and obſerved that they died in a few minutes

time, but without any apparent ſign of convulſions. Theſe experiments, having been often repeated, were conſtantly attended with the ſame event.

Being aſſured, beyond any doubt, that the inflammable air obtained from zinc or iron, and made to paſs through quickſilver, was fatal to animals; I next wiſhed to obſerve, whether it retained the ſame properties when it had paſſed through water; in which caſe the volatile ſulphurous acid, or other vapour, is abſorbed by the water; but, on trying the experiments, I found that the birds died under theſe circumſtances as under the others (though not quite ſo ſoon) ſhewing likewiſe ſome ſigns of convulſion. I introduced ſome of this ſame air that had paſſed through water into a glaſs tube full of quickſilver, by a method which makes the air loſe all its moiſture. The birds died in it in the ſame manner as when the experiment was tried upon water. In all theſe caſes the air after the animals had died in it was ſtill inflammable, nor did its exploding properties ſeem to have been at all diminiſhed.

The inflammable air extracted from zinc, and that extracted from iron, is fatal to animals even after it has been ſhaken in water for a minute's time, or ſomething longer. By ſhaking it a long time, it becomes in ſome meaſure reſpirable; but then it is decompoſed in a great meaſure,

meaſure, and becomes of another kind, although it ſtill preſerves the properties of being inflammable, but in a ſmaller degree,

Not only birds but alſo quadrupeds die in inflammable air (though not ſo ſoon) and ſhew ſome ſigns of being convulſed.

It ſeems very ſtrange, that Mr. SHEELE could breathe inflammable air with impunity, when animals obliged to breathe it were killed in a very ſhort time. Admitting his experiments to be true, there remains nothing to be ſaid, but that the inflammable air in which animals die does not occaſion death becauſe it is conveyed to the lungs, but becauſe it affects ſome other organs of the animal body expoſed to that air, and neceſſary to animal life. It is not impoſſible to occaſion death by affecting the very ſenſible nerves of the noſe; it being well known, that various liquors, as very concentrated volatile alkaly, &c. if they are inſpired through the noſe, immediately affect the ſenſes, and occaſion death if they continue to act upon the *pituitary* membrane.

In order, therefore, to try whether inflammable air killed, only becauſe it was inſpired through the noſe, I ſtopped very accurately the noſes of various birds with ſoft wax, and in this manner I introduced them into receivers

receivers full of inflammable air extracted from zinc, and from iron, through water. The birds died within a few seconds, that is, just as they did when their noſes were unſtopped. Quadrupeds were tried after the ſame manner, and the event was the ſame.

Having in this manner exploded this new hypotheſis, there remained one more, which ſeemed to ſuggeſt a probable reaſon (ſince ſome reaſon there muſt be) for Mr. SHEELE's experiments being attended with reſults ſo different from thoſe of other experimenters. When an animal is introduced into a veſſel of inflammable air, its whole body is expoſed to that air; and it is not yet known by philoſophers what diſorders that fluid may occaſion to the animal frame. It is true that none are obſerved to be produced by other noxious kind of air; but if it be conſidered, that the vapours of ſulphur make a great impreſſion upon frogs, even when thoſe animals do not breathe them, but have their *aſpera arteria* tied up, it will not ſeem impoſſible for the inflammable air, in ſome manner or other, to act upon the body of animals. It may, perhaps, hinder the perſpiration; it may inſinuate itſelf through the pores of the ſkin; in ſhort, its action upon the body ſeems probable till experiments evince the contrary.

I therefore endeavoured to force various four-footed animals to breathe the inflammable air through the mouth only, without immerging their whole bodies into it. I chiefly uſed bladders tied to their mouths, but ſometimes I alſo made uſe of tubes which entered immediately into the wind-pipe. In both caſes the animals died in a very ſhort time: hence it became evident, not only that the inflammable air is pernicious to animal life, but that it does not act on the *body* of an animal; for I kept ſome of them immerged in inflammable air, with the mouth only out of it, and did not perceive any effect hurtful to them.

It being in this manner aſcertained, that the inflammable air could not be breathed by animals with impunity, it ſtill remained to find out the cauſe of Mr. SHEELE's miſtake. I began therefore to breathe the inflammable air contained in bladders, after the manner of Mr. SHEELE. The inflammable air uſed in my experiments was extracted from zinc and from iron by the action of the vitriolic acid, and it was received into bladders that were dry in the inſide, but a little moiſt on the outſide. The quantity of air contained in each bladder was about eighty cubic inches. The air coming out of the mattraſs paſſed through about one inch of water before it went into the bladders. At firſt I breathed

breathed the inflammable air with a kind of fear; but finding that it occaſioned no painful impreſſion, I continued breathing it with courage as long as I could. I breathed in a bladder filled with it eleven times, beginning after a natural expiration. This air when taken out of the bladder, was ſtill inflammable, and being tried with the teſt of nitrous air it gave II−28, III+20.

Before I go farther I muſt explain the formula which I uſe to expreſs the diminution of reſpirable air, or air of other kind, when mixed with nitrous air. My method is as follows: I have a glaſs tube of about eighteen inches in length, and half an inch in diameter, cloſed at one end, and of a conſtant diameter throughout its whole length: this tube has a mark at every three inches, which marks or diviſions I call *meaſures*, and every inch is divided into twenty equal parts; ſo that every *meaſure* is divided into ſixty portions, which I call *parts*. Into this tube, by means of an inſtrument which meaſures always one conſtant quantity of air equal to one meaſure of the tube, I introduce two meaſures of reſpirable air and one meaſure of nitrous air, after which I meaſure the diminution; then I introduce a ſecond meaſure of nitrous air, and again meaſure the diminution. The whole meaſures I expreſs in Roman characters, and the

parts of a meaſure I expreſs in common numbers; for inſtance, when I ſay II−16 and II+10, the firſt expreſſion means, that after having introduced into the tube two meaſures of common air, and one meaſure of nitrous air, the ſpace occupied by the mixture of theſe two airs was two meaſures − 16 parts, or 60ths of a meaſure: and the ſecond expreſſion ſhews that, after having introduced another meaſure of nitrous air, the ſpace occupied was two meaſures + 10 parts. The reaſon and particulars of this method will be given hereafter in a paper expreſsly written upon the method of determining the degree of the ſalubrity of the air by means of nitrous and inflammable air.

Having introduced eight cubic inches of common air into the ſame bladder, I breathed it as long as I could; beginning after a natural expiration as in the experiment above related. I breathed it thirty-four times ſucceſſively, and afterwards found it very much altered, ſo that it extinguiſhed a light many times ſucceſſively. An animal introduced into a veſſel of that air immediately gave ſigns of uneaſineſs: and the air being tried with the nitrous air gave II+20, III+15; whereas, before it had been breathed, it gave with the ſame nitrous air II−15, II+18.

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This experiment ſhews, that the air which remained in the bladder in the firſt experiment was not ſo good as that breathed thirty-four times ſucceſſively. In order to make this experiment with more preciſion, I breathed eighty cubic inches of common air, introduced into the ſame bladder, only eleven times; beginning after a natural expiration. Then I examined this air with the nitrous air, and found that it gave II−13, III+28.

Hence it is plain, that the mixture of inflammable and pulmonary air breathed eleven times is much inferior to common air breathed an equal number of times; ſo that there can remain no doubt but that inflammable air is at leaſt worſe than common air.

Willing, however, to aſcertain this matter ſtill better, I tried to breathe it immediately through a large receiver, partly immerſed in water, and ſwimming in it, ſo that the air within the receiver was of the ſame elaſticity with the external air. For this experiment I made uſe alſo of a glaſs tube bended in two different directions.

The air contained in the receiver was about 250 cubic inches. In all the trials made in this manner, I was never able to breathe the inflammable air more than three times, and even at the ſecond inſpiration I felt a great oppreſſion. As theſe experiments can be depended upon, becauſe they were often and at different times repeated, there

there ſeems to be reaſon enough to ſuſpect, that the bladder might poſſibly alter the nature of inflammable air, and render it more fit for reſpiration, notwithſtanding that the mere contact of the bladder ſeemed not ſufficient to produce ſuch an effect, which is irreconcileable with other facts: yet ſome reaſon muſt certainly exiſt ſufficient to explain Mr. SHEELE's experiments, which directly prove that the inflammable air contained in bladders can be breathed with impunity.

When I breathed this air according to Mr. SHEELE's manner eleven times ſucceſſively, I not only breathed it without any inconvenience, but obſerved that the firſt inſpirations were even pleaſing; more ſo than when I breathed common air. I felt a facility of dilating the breaſt, as if the air was as light as that at the top of high mountains. I never felt a like ſenſation, even when I have breathed the pureſt dephlogiſticated air. I do not think that I was miſtaken in theſe ſenſations, or gave a looſe to imagination, becauſe I was rather prejudiced againſt the inflammable air, after I had ſeen various animals immediately die in it, and I was rather fearful when I firſt began to breathe it: beſides, this facility of breathing it, accompanied with a pleaſing ſenſation, I have conſtantly obſerved in all my experiments upon this ſubject.

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This pleaſure, however, I paid very dear for in another experiment, in which I was near loſing my life. Having filled a bladder of the largeſt ſort with about 350 cubic inches of inflammable air extracted from iron filings through water, which air was not at all diminiſhed by the mixture of nitrous air; I began to breathe it boldly (owing to the encouragement received from the above related experiment), and reſolved to breathe it as long as my ſtrength would permit me, after having made a very violent expiration in order to evacuate the lungs of the atmoſpheric air. Having made the firſt inſpiration I felt a great oppreſſion upon my lungs. Towards the middle of the ſecond inſpiration I heard Mr. CAVALLO, who favoured me with his aſſiſtance in theſe experiments, ſay, that I was become very pale: by this time the objects appeared confuſed to my eyes. Notwithſtanding this, I made the third inſpiration; but now my ſtrength failing, I loſt my ſight intirely, and fell upon my knees. In this ſituation I breathed the air of the room, but my knees not being able to ſupport me, I fell intirely upon the floor. However, in a ſhort time I came to myſelf, ſo as to be able to get up, &c.; but my reſpiration continued to be affected with difficulty and pain, as if I had a great weight upon the breaſt; nor did I perfectly recover before the next day.

It muſt be obſerved, that during this experiment I kept my noſe cloſe ſtopped.

This ſame inflammable air contained in the bladder, which I had breathed three times, was examined in various manners, and was found to be as inflammable as before; it exploded as uſual, when mixed with dephlogiſticated air, but after having been ſhaken in water for a ſhort time, being tried with the nitrous air, it gave III−10, IV−10, whereas before it was not at all diminiſhed. At this time the common air, with the ſame nitrous air, gave II−14, II+10. Hence it appears, that the inflammable air, after being breathed, is rather better than before, becauſe in that caſe it is a little diminiſhed by the addition of nitrous air.

In order to aſcertain whether this alteration was occaſioned by the bladder or no, I made the following experiment, which, having been often repeated, was conſtantly attended with the ſame event. I introduced into a bladder, which was ſometimes moiſt and ſometimes dry, a quantity of inflammable air, extracted as well from zinc as from iron, through water, and having kept it in that ſituation for ſome minutes, beating in the mean while the bladder, to keep the air in agitation, I afterwards took it out, and by the mixture of nitrous air obſerved

ſerved, that it ſuffered no diminution, exactly as it ſuffered none before it had been put into the bladder.

Having aſcertained, in this manner, that the bladders do not in any manner contribute to render the inflammable air extracted from metals better in its nature, there remained no other way of aſcertaining Mr. SHEELE's experiments, and of underſtanding why I had been able to breathe it eleven times, than by ſuppoſing that the air of the lungs, which can never be thoroughly emptied by being mixed with the inflammable air, alters it, &c. It is well known, that in an ordinary expiration about thirty-five cubic inches of air are expelled from the lungs. In a very violent expiration, following a natural inſpiration, the air expelled may amount to ſixty cubic inches. Theſe forty inches of pulmonary air are mixed with the inflammable air, and are expelled from the lungs in proportion to the remaining air that is breathed after that the lungs have been thoroughly emptied. In the expement above related, of the three inſpirations I made into the inflammable air, it may be eaſily ſuppoſed, that twenty inches or more of pulmonary air were joined with the inflammable air, and entered into the bladder. This pulmonary air, although it is itſelf partly phlogiſticated, is however diminiſhed by nitrous air; and when it ſtands in the bladder it is nearly equal to $\frac{1}{17}$th of the inflammable

air of the bladder breathed three times; hence this loſt ten parts by the mixture of nitrous air.

This explanation, which it is neceſſary to adopt after having exploded all the other hypotheſes, is very analogous to the above related experiment of the ſmaller bladder filled with inflammable air which was breathed eleven times ſucceſſively. This air was breathed after a natural expiration, ſo that there ſtill remained in the lungs about ſeventy-five inches of common air. Theſe ſeventy-five inches of pulmonary air, together with the eighty inches of inflammable air, were mixed together during the eleven inſpirations and expirations; hence the air of the bladder was a mixture of nearly equal portions of inflammable and common air; and, accordingly, when tried with the nitrous air, it was found to be much better (though it had been breathed eleven times) than the air of the large bladder breathed three times only, after the lungs had been emptied as much as poſſible.

All the other experiments that I have made in confirmation of this hypotheſis ſeem univerſally to favour it. If a Guinea pig is introduced into a receiver containing 400 cubic inches of inflammable air, or a ſmall bird into only fifty inches of it, and they be left therein till they are dead, that air afterwards will not be ſenſibly diminiſhed

niſhed by nitrous air; but if a much larger animal is introduced into the 400 inches of inflammable air, or a ſmall animal into a few cubic inches of that air, then it will be found to be ſenſibly diminiſhed by nitrous air; and this diminution will be greater as the animal is larger in proportion to the quantity of inflammable air. A larger animal imparts a greater quantity of its pulmonary air to the inflammable air; and the inflammable air will be found joined to a quantity of pulmonary air, which is ſo much the leſs as the animal is ſmaller.

Mr. SHEELE ſays, he found that the inflammable air after being breathed ſome time intirely loſes its inflammability; from whence he concludes, that the lungs, inſtead of imparting ſome phlogiſton to, imbibe it from, whatever ſubſtance it can be extracted. Though all the direct experiments which ſhew that a phlogiſtic principle is continually detached from the lungs, and joins itſelf to the common air, were wanting, ſtill Mr. SHEELE's conſequence could not be drawn, becauſe the experiment is not true. With reſpect to my own experience I may ſafely ſay, that I have always found it inflammable in every circumſtance, even after I had breathed it eleven times ſucceſſively: and I have not only found it inflammable in the bladder, but I have fired it in the act of let-

ting it out of my mouth. In this manner a flame may be produced from the mouth, of various inches in length, and two or more inches in breadth.

But whence does that ſenſation of levity and facility of breathing the inflammable air, which I have deſcribed above, originate? At preſent I can only have recourſe to a mere mechanical cauſe for a ſolution, for I do not obſerve in inflammable air any property that ſeems capable of altering the lungs upon a chemical principle; neither have I obſerved any decompoſition of air, or alteration of the fluids of the animal. It has been obſerved, that inflammable air, after being breathed, comes out of the lungs with the ſame properties it had before. It is alſo known, that inflammable air is not ſenſibly abſorbed by water, at leaſt after a ſhort time. The lungs, or more properly the pulmonary veſicles, are continually moiſtened with fluids; but that air cannot be abſorbed by them, except it be firſt decompoſed. Nothing elſe therefore remains to which we can have recourſe for an explanation of the above mentioned ſenſations, but the well known levity of the inflammable air compared with common air. And indeed the ſenſation I felt when I breathed that air, is like that of a very light fluid which does not oppreſs the lungs, and is hardly felt. This explanation agrees exactly with

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ſome experiments I have made with common air rendered more light by fire. This air I have found may be breathed much eaſier, although not for ſo long a time, as when it is more condenſed. It muſt be ſaid, indeed, that this is occaſioned by another particular cauſe, which has nothing to do with the caſe of the inflammable air, and which cannot be properly examined in this place.

After all, it ſtill remains to be known, why inflammable air, which kills animals ſo ſoon, may be breathed without any oppreſſion, when in a ſmall quantity, *viz.* when it is mixed with common air; and the following experiments, which are very analogous to thoſe related above, will ſhew that the queſtion is not unintereſting.

I introduced 350 cubic inches of common air into a bladder, and after having made as ſtrong an expiration as I could, I applied the neck of the bladder to my mouth, and breathed the air it contained forty times ſucceſſively. Afterwards, having taken the air out of the bladder, I found that it extinguiſhed a light ſeveral times ſucceſſively. It formed various cryſtals with the oil of tartar, but after a very conſiderable time; ſome of theſe cryſtals had the ſhape of needles, others were like flowers: being tried with the nitrous air, it gave II−18, III+18. This air, therefore, was very much phlogiſticated, nor could I poſſibly have breathed it longer than I did, without

out falling upon the ground, as I already felt my ſtrength failing, and the objects appeared confuſed before my eyes. Into ten cubic inches of this air I introduced a ſmall bird, which, as ſoon as it began to breathe it, made various contorſions with its body, and ſeemed to ſuffer a great deal. It died in ten minutes time; whereas another little bird introduced into a like quantity, that is, into ten cubic inches of common air, lived in it fifty-two minutes, nor did it ſhew any ſign of uneaſineſs before it had been in five minutes.

It remains to be accounted for, why the bird could breathe for five minutes longer in the air of the bladder that a man could. It will be ſufficient to conſider, that when a man in this experiment has made the laſt expiration into the bladder, he is in a ſtate of pain, and his lungs are loaden with a ſuperfluous quantity of phlogiſton, which is not communicated to the air of the bladder; whereas nothing of this takes place with the bird, which, beſides its being in vigour, has a quantity of common air in its lungs. This ſeems confirmed by an experiment, which admits of no doubt. Having breathed the air of the bladder as long as I could, I ſtopped the neck of the bladder with my finger, then breathed the common air ſeveral times; and afterwards putting the neck of the

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ſame bladder to my mouth again, I breathed that very ſame air four times ſucceſſively. Now there is no doubt but that a bird could have breathed it much longer: the reaſon of which diverſity ſeems to be the following, *viz* that a ſmall bird is in want of a ſmall quantity of air for every time it breathes, whereas a man is in want of a much greater quantity; hence the air is rendered more eaſily noxious, and unfit for reſpiration. From all which it may be concluded, that we are in want of a certain quantity of common air neceſſary for reſpiration, and for the ſupport of life; and that this air, after being inſpired, comes out of the lungs leſs fit to be breathed a ſecond time.

It has been obſerved, that the inflammable air cannot be breathed when the lungs are emptied of common air as much as poſſible; but that it may be breathed when the lungs are in a natural ſtate, in which ſtate a quantity of common air, equal to about forty cubic inches, is known to exiſt in the lungs of an adult perſon. This pulmonary air is not infected ſo far as to be incapable of being breathed various times, and of ſupporting life. After having made a natural expiration I have with force expelled from my lungs about thirty inches of air into an empty bladder; and this pulmonary air I have generally been able to breathe eight times ſucceſſively,

but never longer. It is true, however, that I breathed it with ſome oppreſſion, even from the beginning, which does not happen when the inflammable air contained in a bladder is breathed, the lungs being in a natural ſtate.

And now it ſeems no longer difficult to give an anſwer to the queſtion propoſed above, and to account for the ſmall difference obſerved in the breathing of the two different kinds of air in the bladders. The inflammable air, when joined to a great quantity of common air, may be breathed ſafely, becauſe there is a quantity of common air ſufficient for various inſpirations, and that the mixture of the two airs may be breathed till this common air is thoroughly infected. But the inflammable air itſelf is not altered nor decompoſed by the reſpiration. Wherefore we muſt conclude, that the inflammable air is not ſuch a kind of air as can by itſelf alone be directly uſeful for reſpiration. It muſt rather be conſidered as if there was nothing of that air in the caſe of the bladder; and indeed it is found by experience, that the pulmonary air itſelf may be breathed eight or nine times in an empty bladder. The not being able to breathe it eleven times ſucceſſively, as was done when there was inflammable air in the bladder, and the feeling an oppreſſion in the firſt caſe and not in the ſecond, muſt be intirely attributed to the want of thirty-five cubic inches of air

expirated,

expirated, which are neceſſary to give the lungs all the neceſſary expanſion; whereas, in the other caſe, the inflammable air ſerves to fill up ſpace, and, together with the common air, contributes to fill the lungs; ſo that the inflammable air, conſidered under theſe circumſtances, and under this point of view, may be ſaid to be uſeful for animal reſpiration. This explanation ſeems moſt evidently demonſtrated by the following experiment. If thirty-five cubic inches of common air are introduced into the bladder, and this air be breathed when the lungs are in a natural ſtate, it will be found, that one may breathe it for twenty times or longer; whereas, when the bladder was empty, it could not be breathed more than nine times at moſt.

Before I finiſh this paper it will be proper to mention another cauſe, which, perhaps, alſo contributes to render the inflammable air of the bladder leſs noxious: this is the levity of the inflammable air itſelf with reſpect to common air, which hinders the inflammable mixing with the common air. The inflammable air ſwims continually upon the common air, juſt as æther ſwims upon water; and the inflammable air ſwims ſtill better than æther, becauſe it is much lighter in compariſon than æther. Various experiments made upon volatile ſubſtances have convinced me of this truth. If equal quan-

tities of common and inflammable air, or dephlogiſti-cated and inflammable air, are put into a tube, and two birds are introduced in it, ſo that one of them may ſtand at the top, and the other at the lower part of the inverted jar; it will be found, that the firſt mentioned of theſe birds not only will die conſiderably ſooner than the other, but will ſhew ſigns of uneaſineſs as ſoon as it is come to that place.

The inflammable air, therefore, when breathed toge-ther with a conſiderable quantity of common air, muſt always ſwim at the top of it, filling the cavity of the wind-pipe, &c. while the common air occupies the lower place, and filling the ſmalleſt pulmonary veſicles is ſub-ſervient to the ordinary functions of the lungs.

Here I put an end to my obſervations upon inflam-mable air conſidered with reſpect to reſpiration; but I beg leave to add a few words reſpecting a property of the inflammable air, which, as far as I know, has been over-looked by the moſt diligent obſervers.

I mean here to ſpeak of ſuch inflammable air as is ex-tracted from metals, by means of oil of vitriol, eſpecially that extracted from iron and zinc. The air of theſe me-tals, when preſented to the flame of a candle, not only burns with a whitiſh flame inclining to green (as is well known); but exhibits a kind of ſparks or explоſions which

which may be eaſily diſtinguiſhed between the body of the flame by their vivid light. Theſe ſparks, which are of a vivid colour, dart in every direction. They might be eaſily taken for thoſe ſparks that are emitted from red-hot iron; or they might be compared to very ſmall grains of gunpowder, if theſe were inflamed ſucceſſively, and without ſmoke; or they might even be compared to charcoal that ſparkles, but without any noiſe. This phenomenon ſeems very intereſting, as it reſpects the nature of the inflammable air itſelf. What ſeems to me moſt ſingular is, that this appearance forms a diſtinctive character between the inflammable air of metals, and that extracted from animal or vegetable ſubſtances; at leaſt I may ſafely ſay, that I never found the inflammable air of animal or vegetable ſubſtances ſparkle like that extracted from metals. In ſeveral of the former kinds of air I could obſerve no ſparkling at all; in others the ſparks were ſo few that they might be conſidered as nothing in compariſon to the ſparkling of the inflammable air from metals.

The inflammable air of metals itſelf, if left in contact with water for a long time, or ſhook in it till it becomes leſs inflammable, will in great meaſure loſe its ſparkling property, and at laſt loſes it intirely, when it is become in a ſtate of being hardly inflammable. I have obſerved,

that the inflammable air is the more difficult to be decompoſed, by being ſhaken in water, as the number of the ſparks it ſhews when burning is greater; and according to this number of ſparks, the inflammable air makes greater or weaker exploſions when mixed with the dephlogiſticated air; ſo that it ſeems proved by expements, that the phlogiſtic principle is more fixed and in greater quantity combined with the inflammable air of metals, than with that of vegetable or animal ſubſtances. I do not mean to deny the poſſibility of finding other ſpecies of inflammable air extracted from other ſubſtances beſides metals which may explode like that extracted from metals; but I only ſay, that in thoſe caſes the inflammable air will alſo ſparkle more, and will be found leſs eaſy to be decompoſed by water. There are other ſubſtances that give the inflammable air in great quantity, and which cannot be conſidered as animal or vegetable ſubſtances, but come rather near the nature of metals; as, for inſtance, the ſpathoſe iron, from which I extract a good deal of inflammable air by the action of fire only, applied to a mattraſs. But the metal in this ſubſtance is not in its pure ſtate, and it may be conſidered rather as a calx of iron than true iron. Accordingly, this air can hardly ſparkle at all; it explodes more like

the inflammable air of vegetable or animal bodies than that of metals, and it is eaſily decompoſed in water.

This property of inflammable air of metals which I have diſcovered, throws great light upon the analyſis of the decompoſition of that air which I have made in two different ways. The firſt is to fire it together with common or dephlogiſticated air, in veſſels filled with very pure quickſilver, and alſo in veſſels filled with diſtilled water. The ſecond method is to decompoſe it by ſhaking it in pure diſtilled water. In the firſt proceſs a great number of experiments are required in order to obtain a ſenſible reſiduum; beſides, the igneous part is loſt. The ſecond method requires an exceedingly long time, but it is the moſt complete; for which reaſon I have uſed it for the decompoſition of other kinds of air.

XXV. *On the Variation of the Temperature of boiling Water.* By Sir George Shuckburgh, *Baronet, F. R. and A. S. and Member of the Academy of Sciences and Belles Lettres at* Lyon.

Read March 11, 1779.

THE heat of boiling water having for ſome years been uſed as one of the terms for graduating the ſcale of thermometers; together with the particular attention the Society has lately given (*vide* the Report of the Committee, Phil. Tranſ. vol. LXVII.) to this branch of inquiry, and I may add the ſingular ſucceſs with which this age and nation has introduced a mathematical preciſion, hitherto unheard of, into the conſtruction of philoſophical inſtruments, will render it unneceſſary for me to ſay more in reſpect of the following experiments, than ſimply to lay them before the Royal Society.

That the heat of boiling water was variable, according to the preſſure of the atmoſphere, ſeems to have been known to FAHRENHEIT as early as the year 1724[a].

(a) *Vide* Phil. Tranſ. N° 385. wherein is propoſed a curious project of determining the weight of the atmoſphere by means of a thermometer alone, under the title of "Barometri novi deſcriptio."

A few

A few years after this, Meſſieurs LE MONNIER and CASSINI (*Mem. de l'Acad. des Sc. pour* 1740) made ſome deciſive obſervations, to ſhew that this quantity was very conſiderable. It was left, however, for Mr. DE LUC to make a much more compleat ſeries of experiments, which he has deſcribed and reduced into ſyſtem in his *Recherches ſur la Variation de la Chaleur de l'Eau bouillante*. It remained only that theſe ſhould be verified. Towards the latter end of the year 1775 I had an opportunity of repeating theſe obſervations with a ſmall pocket thermometer of about ſix inches long, made by Mr. NAIRNE; an inſtrument, it muſt be confeſſed, not very accurate for ſuch an examination, but with which I thought, however, I could obſerve to within a quarter of a degree; my object at that time, amidſt a variety of other philoſophical purſuits, being to aſſure myſelf that the variation took place, rather than critically to examine the quantity of it. I ſhall relate theſe obſervations, as the reſult of them upon my return to England led me to ſome more accurate.

Table

Table of obſervations of the boiling-point, made in a journey over the Alps.

Place of obſervation.	Height of the barometer.	Heat of boiling water by obſervat.	Heat of boiling water by Mr. DE LUC's thermometer	Diff.	Difference in $\frac{1}{100}$ of total.
	Inches.	°	°	°	
Bologna,	30.21	213.5	213.5	−0,0	0
Geneva,	28.60	210.4	210.9	−0,5	−16
Modane,	26.61	207.3	207.4	−0,1	− 2
Lannebourg,	25.75	205.1	205.9	−0,8	− 9
Mount Cenis,	24.03	201.2	202.6	−1,4	−11
Ditto,	23.91	201.1	202.4	−1,3	−10
				Mean	$\frac{9}{100} = \frac{1}{11}$

The ſecond column gives the height of the barometer at the time of obſervation; the fourth, the heat of boiling water deduced from Mr. DE LUC's rules, compared with the lowermoſt obſervation, or that under the greateſt preſſure; the ſixth gives the difference between the theory and the experiment in the motion of the boiling point in hundredth parts of the whole ſpace deſcribed: from whence it might be concluded, that the motion or variation of the boiling point with a given variation in the preſſure of the atmoſphere was $\frac{9}{100}$ or $\frac{1}{11}$ greater

greater than by the theory alluded to[b]. But these were but gross experiments, and perhaps unworthy of such a competition. They induced me, however, to make the following. In the beginning of last year (1778) with the assistance of Mr. RAMSDEN, I procured a most excellent thermometer, every way adapted for this purpose. It was about fourteen inches long, but the interval between freezing and boiling only $8\frac{1}{4}$ inches[c], and though every degree was something less than the $\frac{1}{10}$th of an inch, yet, by means of a semi-transparent piece of ivory, which applied itself close behind the glass tube, sliding up and down in a groove cut in the brass scale for that purpose, carrying a hair-line division, at the extremity of which was a vernier dividing each de-

(*b*) The same instrument immersed in snow just melting at the top of Mount Cenis fell to 32°, the point of freezing observed at the level of the sea.

(*c*) It may possibly be suggested, that if this interval had been greater, *viz.* 20, 30, or 40 inches, I should have had a much larger scale and more convenient instrument; but in this, as in most other mechanical contrivances, our progress beyond certain limits is prevented; for if the perpendicular height of the column of quicksilver be much increased, the weight of it will be such as to distend the ball, and the instrument may differ from itself in a vertical and horizontal position by half a degree, as I have seen in a tube only fifteen inches long; and if this circumstance be endeavoured to be corrected by making the bulb of the thermometer thicker, its sensibility will be proportionably diminished. If my experience were to lead me to conclude any thing, I should consider a tube of a foot long as a *maximum*, and the bore of such a diameter as to admit a ball of a quarter or one fifth of an inch.

gree into ten; with, moreover, a lens of an inch focus; this apparatus being made moveable first by the hand, and more delicately by means of a micrometer screw, whose head was divided into twenty-five divisions, each equal to the fortieth of a degree (for so truly cylindrical was the tube, which had been with care expressly selected from a great quantity of glass, that the divisions in the neighbourhood of the freezing point did not differ from those near the boiling point by so much as $\frac{1}{40}$th of a degree, and this variation appeared in other parts of the tube strictly uniform, as was found by breaking the column of mercury); by means I say of this apparatus I was enabled to read off any height of the thermometer to within $\frac{1}{500}$th of a degree. The vessel, in which the water was boiled, which was always spring water, was a cylindrical tin pot, 13 inches high and $4\frac{1}{4}$ inches wide, with a top something resembling that described in Mr. DE LUC's work, contrived to carry off the steam without incommoding the observer, with a waste pipe for the superfluous water in boiling, which might otherwise fall upon the fire and extinguish it. The ball of the thermometer was immersed to within $2\frac{1}{2}$ inches of the bottom of the vessel, and $10\frac{1}{2}$ inches below the surface of the water, so that as near as might be the whole column of mercury was exposed to the heat of boiling water,

there

there being only 15° or 20° of the ſcale, equal about $\frac{1}{50.0}$ part riſing out of the water expoſed to the temperature of the ſteam, which in one or two experiments was found to be 180° or 190°, ſo that the correction for this defect of heat would only amount to a very few hundredths of a degree, perhaps about ,04 or 05, which, as the inſtrument was expoſed to the ſame circumſtances as near as might be in all the obſervations, I have taken no notice of. I thought it neceſſary to ſay thus much reſpecting the preciſion of the inſtrument and the apparatus, and ſhall now relate the obſervations at length.

Table of the obſervations.

No.	Place of obſervation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
1	Summit of Snowdon in Carnarvonſhire, Aug. 15, 1778,	26,487	46	26,498	207, 8 206,38	207,07
2	Top of Carn Cwm Gafr, Aug. 16, 1778,	27,274	62	27,241	208,27 208,24 208,28 208,19 208,29	208,64
3	In the deſcent from ditto,	27,957	51	27,954	209,28½ 209,37	
					209,39 209,32 209,32 209,40	209,87

Table of observations continued.

Number.	Place of Observations.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
4	Shuckburgh, Dec. 3, 1778,	28,360	44	28,377	210,17½ 210,20½ 210,20½ 210,21½	210,50
5	Ditto, Oct. 23, 1778,	28,690	47	28,699	211, 8 211,10 211,11 211,13 211,12 211,10	211,27
6	Ditto, Sept. 27, 1778,	28,910	54	28,898	211,19½ 211,20½ 211,24½ 211,17½ 211,14½	211,50
7	Ditto, Nov. 7, 1778,	28,990	47	28,999	211,23 211,24 211,25 211,23	211,60
8	Llanberis, Carnarvonshire, Aug. 13, 1778,	29,477	60	29,447	212,22	212,55
9	London, Jan. 27, 1779,	29,790	45	29,805	212,35 212,40 212,42 212,36 212,36 212,38 212,39	212,95

Table

Table of Obſervations continued.

Number.	Place of obſervation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
10	London, Jan. 28, 1779,	29,990	44	30,008	213, 5	
					213, 8	
					213,10	
					213, 9	
					213,18	
					213,10	213,22
					213, 6	
					213, 6	
					213,11	
					213,12	
					213, 8	
11	Ditto, June 10, 1778,	30,250	64	30,207	213,20	
					213,23	
					213,24	213,58
					213,26	
					213,23½	
12	Ditto, Dec. 27, 1778,	30,468	43	30,489	214, 5	
					214, 9	
					214, 6	
					214, 2	
					214, 5	214,15
					214, 7	
					214, 7	
					214, 5	
13	Ditto, Dec. 24, 1778,	30,750	46	30,763	214,18	
					214,12	214,37
					214,15	

Table

Table of obſervations continued.

Number.	Place of obſervation.	Height of the barometer.	Therm. at.	Barometer reduced to the heat of 50°.	Boiling point.	Mean boiling point.
		Inch.	°	Inch.	° 40ths.	° 100ths
14	London, Dec. 25, 1778,	30,838	47	30,847	214,35	
					214,32	
					214,33	
					214,34	214,83
					214,35	
					214 31	
					214,33	
15	Ditto, at the Adelphi Wharf, 3½ feet above high water, Dec. 26, 1778,	30,948 (*d*)	47	30,957	214,35	
					214,39	
					214,40	
					214,38½	
					214,41	214,15
					214,37	
					214,41	
					214,40	
					214,40	
					214,34	

The numbers in the fifth column are the corrected heights of the barometer reduced to one and the ſame temperature, *viz.* 50°, which was neceſſary in order to have the true proportion of the preſſure of the atmoſphere, whoſe influence ſeems to have ſo conſiderable a

(*d*) This is the greateſt height of the barometer that I have ever known, and, as far as I have been able to collect the higheſt point, that it has ever been ſeen to ſtand at in any country where obſervations have been made and recorded, ſince the firſt invention of this ingenious inſtrument.

ſhare

ſhare in the heat of boiling water. Column the ſixth ſhews the height the thermometer ſtood at in water boiling very faſt in degrees and 40ths of a degree; where it will be ſeen, that the obſervation was repeated, as the heat is given of every ſeparate trial, which is, perhaps, the beſt criterion of the confidence that is to be placed in the mean reſult, ſhewn in column the ſeventh expreſſed in hundredths of a degree.

Having collected this ſeries of experiments, I was anxious to ſee how far they correſponded with Mr. DE LUC's, and upon compariſon of N° 1. and N° 15. I found that the decreaſe of the boiling heat was $\frac{147}{100}$ greater than the rules admitted of from an alteration of the preſſure of the atmoſphere of $4\frac{1}{2}$ inches. This difference led me into an examination of all my obſervations, to ſee how far they were conſiſtent with themſelves; how far they diſagreed from Mr. DE LUC's; and, laſtly, what general concluſion might be drawn from them.

To avoid in ſome meaſure, or at leaſt correct, the errors of obſervation, the mean of N° 1. and N° 2. the mean of N° 6. and N° 7. and of N° 14. and N° 15. was taken inſtead of either obſervation ſeparately; the firſt and third of theſe means as two extreme terms, and the ſecond as an intermediate one: with theſe it was very eaſy by interpolation or proportion to deduce any other

intermediate term, and consequently from the mean of six to examine all the fifteen observations. Thus, on a comparison of N° 1. and 2. with N° 14. and 15. the mean motion of the boiling point in that interval for one inch of the barometer (*viz.* when the mercury stands at 28,886 inches) is = 1°,743; according to Mr. DE LUC this is 1°,65. By a similar comparison of N° 1. and 2. with N° 6. and 7. the mean motion of the boiling point in that interval (*viz.* at 27,968 inches) for one inch is = 1°,779; by Mr. DE LUC = 1°,73. And, lastly, comparing the mean of N° 6. and 7. with the mean of N° 14. and 15. the mean motion of the boiling point in this interval (*viz.* at 29,925 inches) for one inch is = 1°,709; by Mr. DE LUC = 1°,59. It should follow then, that, within the limits of my experiments, the alteration or motion of the boiling point is greater (*o*) by $\frac{1}{15}$ than from that gentleman's observations, that the heat of boiling water is not directly in the simple ratio of the height of the barometer, nor yet is the progression so rapid as Mr. DE LUC observed it. It may be somewhat satisfactory to see the observations collated and compared.

(*o*) It is true, that my observations in Savoy give this difference + $\frac{1}{16}$ and those of Mr. LE MONNIER equal + $\frac{1}{7}$. (Vide *Recherches sur l'At.* § 964.); and though, perhaps, neither the one nor the other are intirely unexceptionable, they tend, however, something to confirm, although alone they may be unable wholly to support, such a supposition.

N°

N°	Height of the barometer.	Boiling point by observation.	Boiling pt. by Mr. DE LUC's rules.	Error.	Boiling point by interpolation from N°s 1, 2, 6, 7, 14, and 15.	Error.
	Inch.	°	°	°	°	°
1	26,498	207,07	208,54	+0,47	207,18	+0,11
2	27,241	208,64	208,84	+0,20	208,52	−0,12
3	27,954	209,87	210,03	+0,16	209,80	−0, 7
4	28,377	210,50	210,81	+0,31	210,56	+0, 6
5	28,699	211,27	211,34	+0, 7	211,11	−0,16
6	28,898	211,50	211,67	+0,17	211,47	−0, 3
7	28,999	211,60	211,85	+0,25	211,64	+0, 4
8	29,447	212,55	212,74	+0,19	212,42	−0,13
9	29,805	212,95	213,15	+0,20	213,03	+0, 8
10	30,008	213,22	213,47	+0,25	213,49	+0,27
11	30,207	213,58	213,79	+0,21	213,71	+0,13
12	30,489	214,15	214,23	+0, 8	214,19	+0, 4
13	30,763	214,37	214,66	+0,29	214,65	+0,28
14	30,847	214,83	214,79	−0, 4	214,79	−0, 4
15	30,957	214,96	214,96	,0	214,96	,0

I ſhall make no deductions from this compariſon, but leave them to the leiſure of the reader. It will, however, probably be inquired, how the thermometer came to ſtand at 213° in boiling water, when the barometer was about 30 inches, 212° being the degree by which that heat is expreſſed on FAHRENHEIT's ſcale? The anſwer is eaſy: it was an error in the making of the inſtrument, and, I believe, a pretty general one. There was

alſo a ſimilar error of ſomething more than one quarter of a degree in laying down the freezing point ſo that the fundamental interval between freezing and boiling, when the barometer ſtands at 29,8 inches (the mean height at London) was 180°,71 inſtead of 180°; by this means each diviſion was in fact $\frac{1}{180}$th part leſs than a degree: this ſmall correction may therefore eaſily be applied, if thought neceſſary, but I have taken no notice of it.

I will now add a general table for the uſe of artiſts in making this inſtrument, both according to my own obſervations, and thoſe of Mr. DE LUC, that the preference may be given as it ſhall be thought due; not that it is a matter of any great conſequence which is made uſe of under ſmall variations of the atmoſphere; but even under theſe circumſtances, I flatter myſelf, that the object of this paper will be ſufficiently obvious to all who wiſh to verify a new theory, or aim at accuracy in theſe days of preciſion.

Height

Height of the barom.	Correct. of the boiling point.	Diff.	Correct. according to Mr. DE LUC.	Diff.
26,0	−7,09		−6,83	
		,91		,90
26,5	−6,18		−5,93	
		,91		,89
27,0	−5,27		−5,04	
		,90		,88
27,5	−4,37		−4,16	
		,89		,87
28,0	−3,48		−3,31	
		,89		,86
28,5	−2,59		−2,45	
		,87		,83
29,0	−1,72		−1,62	
		,87		,82
29,5	−0,85		−0,80	
		,85		,80
30,0	0,00		−0,00	
		,85		,79
30,5	+0,85		+0,79	
		,84		,78
31,0	+1,69		+1,57	

XXVI. *Account of a new Kind of inflammable Air or Gaſs, which can be made in a Moment without Apparatus, and is as fit for Exploſion as other inflammable Gaſſes in uſe for that Purpoſe; together with a new Theory of Gunpowder.* By John Ingen-Houſz, *Body Phyſician to their Imperial Majeſties, and F. R. S.*

Read March 25, 1779.

THE important diſcoveries on different kinds of air have opened a new field for one of the moſt pleaſing and intereſting ſcenes which ever were expoſed to the contemplation of philoſophers, and therefore could not fail of exciting in almoſt every lover of natural knowledge a decided curioſity to ſee the purſuit of ſuch ſtriking novelties, and in many an almoſt irreſiſtable temptation to imitate them, and to purſue farther the road already ſo far opened by the Rev. Dr. PRIESTLEY, Abbé FONTANA, Mr. LAVOISIER, and many other learned and ingenious men.

Who can, indeed, without the greateſt ſatisfaction, look (amidſt many other objects of admiration) upon the diſcovery

discovery of that new aerial fluid, which in purity and fitness for respiration so far surpasses the best atmospherical air, that an animal protracts his life five times as long, or even more, in it than in common air of the best quality?

Dr. PRIESTLEY, the first who discovered this wonderful fluid, extracted it from bodies which we should rather have suspected to have contained deleterious exhalations. He afterwards found it existed in many other bodies in which the most accurate observer never found any thing of an approaching nature.

When I consider the immense quantity of this pure aerial fluid, called by Dr. PRIESTLEY with so much propriety *dephlogisticated air*, which exists as it were in a solid state in nitre, in calcined metals, and even in some other of the most common substances, I cannot express the greatness of my expectation, as a physician, from such an important discovery (*a*). I flatter myself, that ere long an easy and cheap method will be discovered by which such quantities of this beneficial air may be ob-

(*a*) Since Dr. PRIESTLEY's last publication on air, he discovered, that the same water which, if examined immediately, gives only a small quantity of bad air, yields spontaneously about ten times the quantity of pure dephlogisticated air after standing some time exposed to the sun. Of this I was an eye-witness. The important discoveries of Abbé FONTANA upon this subject, which he shewed me when I was last in *Paris* a year and a half ago, will soon be published by himself. He extracted this wonderful aerial fluid from different kinds of water by boiling them over a fire.

tained as will ſerve to cure ſeveral diſeaſes which reſiſt the power of all other remedies, and ſo prolong, as it were, human life. We may expect, with ſome degree of confidence, that this new element, when it ſhall be uſed for the benefit of reſpiration, will be found more fit than the beſt common air to free our body from that quantity of phlogiſton or inflammable principle which ſeems to exiſt ſometimes in too great a quantity in the maſs of our blood; or from which it ſeems ſometimes, as it were, to be let looſe in too great abundance, producing, perhaps, in conſequence fevers and other ſymptoms, the cauſes of which have not yet been clearly elucidated by the beſt medical writers.

This dephlogiſticated air, free from the inflammable particles with which the beſt common air is always infected, will probably be found capable of abſorbing a greater quantity of thoſe phlogiſtic particles with which the air coming from our lungs is always found to be pregnant, and thus of ventilating, as it were, much more expeditiouſly the maſs of our blood of that which a conſtant exertion of the organs of reſpiration is not always able to free it from in a ſufficient quantity.

Theſe important purſuits have led Dr. PRIESTLEY to the diſcovery of one of the benefits, and perhaps the principal, we derive from reſpiration; that function of the

animal œconomy which is ſo important, that without its conſtant influence an animal, once born, has but a few moments to live.

The criterion of the degree of goodneſs of reſpirable air, by the quantity which is abſorbed or deſtroyed by the addition of nitrous air, is one of thoſe uſeful diſcoveries of Dr. PRIESTLEY's from which mankind will, perhaps, hereafter reap a conſiderable benefit.

The diſcovery of the various kinds of inflammable airs or gaſſes becoming powerfully explosive, when they are mixed with a ſufficient quantity of common air, and ſtill more ſo when they are combined with dephlogiſticated air, is one of thoſe improvements in natural philoſophy which, giving occaſion to various amuſing and intereſting experiments, have caſt at the ſame time a new light upon ſome powerful agents, whoſe miſchievous force was known, though their nature was ſtill in the dark.

As thoſe inflammable airs have been of late years one of the principal philoſophical amuſements, I intend to lay before the Royal Society an eaſy method of producing, without any trouble or particular apparatus, ſuch quantity of an inflammable air or gaſs as may be required; to which I ſhall ſubjoin ſome conſiderations which have occurred to me about the nature of that wonderful ſubſtance gunpowder, which conveys death at ſuch an immenſe diſtance by its almoſt irreſiſtible explosive force,

force, by which it propels metallic balls of a conſiderable weight. To inveſtigate the nature of this awful ingredient, by which the fate of empires is decided in our days, cannot be indifferent to any body who takes delight in inveſtigating natural cauſes.

Being at Amſterdam in November 1777, Meſſieurs AENEAE and CUTHBERTSON, two ingenious philoſophers of that city, were ſo good as to ſhew me ſome curious experiments with explofive and inflammable airs of different kinds. They produced an inflammable air, by mixing together equal quantities of oil of vitriol and ſpirit of wine, and applying heat to the phial containing the compound. A great quantity of white vapour was extricated, which, paſſing up the inverted receiver filled with water, ſettled at the top and depreſſed the water, as other airs do. This air ſoon became clear, the white fumes being abſorbed by the water. This air was eaſily lighted in an open cylindrical glaſs, and burnt almoſt as clear as a candle, the flame deſcending gradually lower and lower till it reached the bottom. A very little quantity of this air mixed with common or dephlogiſticated air, for inſtance, one fourteenth or one tenth part, and kindled by an electrical ſpark, exploded with a very loud report, and ſhattered the glaſs to pieces in which it was kindled, when it did not find a ready vent.

They

They had contrived a kind of a piſtol for the purpoſe, conſiſting of a ſtrong cylindrical glaſs tube with a piſton adapted to it. To the end of this tube was fixed a braſs barrel, like that of a common piſtol: into this barrel a braſs bullet was put looſe, ſo that the barrel was placed a little above the level, to prevent the bullet rolling out. The barrel was directed to a board of oak at eight or ten feet diſtance. A proper quantity of common and inflammable air (produced in the manner above mentioned) being drawn into the glaſs tube by means of the piſton, it was fired by directing an electrical exploſion through it. The exploſion was very loud: the ball hit the board with ſuch a force that it made a ſtrong impreſſion in it, and recoiled with a conſiderable force, ſo as to hit the wall behind us, and to put us in ſome danger of being hurt by its rebounding force.

The ſame gentlemen told me, that this inflammable air had in ſome reſpects the advantage over the inflammable airs extracted from metals by the vitriolic or marine acid, and that extracted from mud or marſhes; becauſe this air being heavier than either of theſe airs, and even than common air, is not ſo eaſily loſt out of an open veſſel; and, that when it eſcapes into the open air, it agreably perfumes the room with the ſmell of *ſpiritus vitrioli dulcis* or æther; whereas the other inflammable airs,

which from their leſs ſpecific gravity eſcape eaſily into the common air, yield an offenſive, diſagreeable ſtench.

Mr. AENEAE, having examined the ſpecific gravities of the different inflammable airs compared with common air, favoured me with the following reſult of his inquiries.

A veſſel, which contained the weight of 138 grains of common air, contained 25 grains of inflammable air extracted from iron by vitriolic acid, and 92 grains of inflammable air extracted from mud or marſhes, and 150 grains of that extracted from oil of vitriol and ſpirit of wine.

I was much pleaſed with the above mentioned experiment, and immediately thought that the operation of extracting this inflammable air or vapour could be diſpenſed with by employing vitriolic æther, which in reality is contained in the vapour expelled by heat from oil of vitriol and ſpirit of wine, which vapour, condenſed in the proceſs of diſtillation, yields æther. But I reſolved to defer making the experiment till I ſhould arrive in London, where I intended to make ſome ſtay to ſee my old friends, and acquire what medical and philoſophical knowledge I could.

Having arrived in this capital in the beginning of January 1778, I loſt no time in purſuing my idea. For

this purpose I poured some drops of æther into a strong glass tube, and directed an electrical explosion from a Leyden phial through it; but, to my mortification, no explosion happened. I then threw a bit of cotton, dipped in æther, into the same tube, but it would not take fire. Though these first trials proved unsuccessful, I was too much persuaded, that in some way or other it must succeed, to be discouraged. I tried it in several different ways, and at last, before the end of January, I succeeded once or twice in producing a loud explosion, by throwing into the tube a little bit of paper dipped in æther. But as the experiment often failed (of which I could then find no reason), I did not venture to shew it to my friends till I had hit upon a method, the certainty of which would prevent my being exposed to some confusion by exhibiting an experiment which was so apt to fail. However, I told Sir JOHN PRINGLE, Mess. NAIRNE and BLUNT, and some few others of my friends, early in the spring, that I had found out a method of firing an inflammable air pistol without being at the trouble of making inflammable air in the ordinary way; as I produced it at pleasure in an instant, without any trouble or apparatus; and that I would shew them the experiment as soon as I was sure of succeeding constantly. In the mean time I continued to produce this air before my acquaintance in

the way I had ſeen it produced at Amſterdam. Soon after, hitting upon better and ſurer methods of ſucceeding, I began to ſhow it to thoſe who came to viſit me, and in the beginning of the ſummer I made no ſcruple of ſhewing it to every body.

The reaſons why I did not ſucceed in the beginning I found afterwards to be, either that I employed too great a quantity of æther, or that the air or vapour of the æther was not thoroughly incorporated with the other air; for the ſame numbr of drops of æther poured into the air piſtol, which would not produce an explosion when the piſtol was not ſhaken, made a very loud one when it was forcibly agitated.

The ſureſt method of ſucceeding I find to be the following: I dip a ſmall glaſs tube, open on both ſides, and the bore of which is one twelfth of an inch in diameter into a phial containing æther, and when two or three drops of the liquid have entered the tube I apply my finger to the upper end of it, to keep the liquor ſuſpended. I take the tube out of the phial, and thruſt it immediately into a ſmall *caoutchouck*, or elaſtic gum bottle: this being done, I withdraw my finger from the tube, and take it out of the *caoutchouck*; thus the little quantity of æther, ſuſpended in the end of the tube, is dropped into the *caoutchouck*, the neck of which is to be immediately

immediately inverted into the orifice of the air piſtol, and, after giving it a gentle ſqueeze, withdrawn out of it: after which, a bullet or a cork is to be thruſt into the mouth of the piſtol, when it is ready for firing. This whole operation may be performed in the ſpace of five or ſix ſeconds.

The conſiderable force of explоſion, and the loud report of the ordinary inflammable airs, induced Mr. VOLTA, of Como, to believe, that theſe airs might, perhaps, become a ſubſtitute to gunpowder. If this expectation had been well founded, the greateſt *deſideratum* would, I think, have been to find out a way to produce ſuch air at any time without trouble, and to carry it about in as little compaſs as poſſible: which two conditions I ſhould have pretty nearly fulfilled, as all the inflammable air requiſite for the explоſion of the piſtols contrived by Mr. VOLTA is contained in the bulk of one ſingle drop of æther; which drop, poured in the piſtol itſelf, is full ſufficient to produce a very powerful explоſion. But how far this expectation of Mr. VOLTA, as to the uſe of inflammable air in offenſive arms, is well grounded, I will attempt to explain in the ſubſequent part of this paper.

This inflammable air (which, perhaps, might more properly be called vapour, as it is abſorbable by water) has

has the principal properties of the other inflammable airs, *viz.* it will not inflame but where it is in contact with common or dephlogisticated air; and therefore only takes fire at the top of the veſſel containing it, the flame going gradually downwards till the whole is conſumed, if the veſſel is of a cylindrical form, and pretty wide. If it is diluted with common air, but not ſufficiently, it will burn as other inflammable air without exploding. Being ſufficiently diluted with common air, eſpecially with dephlogiſticated air, it explodes with a very great report and a conſiderable force. It is unfit for reſpiration in a concentrated ſtate, and is as mortal for an animal plunged into it as any other of the inflammable airs; though in a diluted ſtate it ſeems to be rather pleaſant to the organs of reſpiration.

It differs in ſome reſpects from the common inflammable airs; as, for inſtance, it is much heavier, as is already obſerved, than any of the other inflammable airs, and even than common air. It does not inflame or explode with ſo ſmall a ſpark of electrical fire as the other inflammable airs, requiring the exploſion of a Leyden phial to be fired with certainty, though one ſingle inch of coated glaſs will be ſufficient; ſo that a Leyden phial, containing twelve or fourteen ſquare inches of coating, may fire an air piſtol loaded with this kind of air ſeveral times,

times, if the charge be divided by taking it out with a ſmall glaſs tube, fitted up in the manner I deſcribed in the paper I had the honour of laying before the Royal Society laſt year, and which is inſerted in the ſecond part of the 68th vol. of the Phil. Tranſ.; in which paper I deſcribe a kind of an electrical match to light a candle with. This air being in contact with water is abſorbed by it in a few days, though the water be not ſtirred: and much ſooner loſes its inflammability by contact with water than the other inflammable airs do. However, I found that in ſuch a ſituation this air had not yet loſt all its explofive force in ſix days, though the water in which the cylindrical glaſs, ten inches long and one in diameter, was inverted, after I had poured into it five or ſix drops of æther, had gradually aſcended till, on the third day, it reached to the half of the height of the glaſs: ſo that it ſeems as if theſe drops of æther, by their expanding in the form of air, had forced out half of the common air contained in the glaſs, in the place of which the water afterwards aſcended in proportion as the air or vapour generated by the æther was abſorbed by the water.

That this air is ſpecifically heavier than common air, and does not readily incorporate with it, is eaſily demonſtrated by the following experiment. I poured five or ſix drops of æther into a cylindrical glaſs, ten inches long

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and one inch in diameter: the æther being ſoon evaporated, I clapped my hand upon the mouth of the glaſs, and inverted it to incorporate the air generated by the æther with the common air; after which I left the glaſs open during half an hour, when I dipped in it a piece of wax taper burning, ſtuck upon a bended wire. As ſoon as the taper reached the brim of the glaſs, a flame burſt out with a weak exploſion. In a quarter of an hour I again thruſt the wax taper into the ſame cylinder, and no flame was obſerved till the wax taper reached the place where the flame ended the firſt time. This ſecond exploſion did not ſet fire to the whole at the bottom of the glaſs. I again waited a quarter of an hour, and then again thruſt the wax taper into it, by which the remainder of this inflammable gaſs, which had remained ſettled all that time at the bottom, was exploded (*b*).

(*b*) It is remarkable, that æther, the moſt volatile liquor yet known, and ſo apt to ſpread itſelf through the air by a quick evaporation that a drop of very fine æther, which falls from the height of a few feet, is quite evaporated before it reaches the ground, and no glaſs ſtopper of itſelf is able to keep it from evaporating: it is remarkable, I ſay, that notwithſtanding this extreme volatility, the air or vapour, into which it changes by evaporation, ſhould be ſo far from participating of the ſame volatility, that it may be kept hours together in an open cylindrical veſſel without evaporating, mixing with the common air, or loſing its inflammability; ſo that this ſubſtance ſeems to undergo a ſudden metamorphoſis, and to change in an inſtant from the lighteſt of all liquors to one of the heavieſt of aerial fluids, fixed air, which being one half heavier than common air, according to the experiments of Mr. CAVENDISH, is probably much heavier than this æther air.

I found

I found that æther, in which as much urinous phoſphorus is diſſolved as will make it luminous in the dark, when ſome drops are poured upon water; is very briſk in taking fire, when employed for an inflammable air piſtol; but that the experiment, when repeated, will be apt to fail, becauſe the phoſphoric acid which remains in the piſtol, and by its nature attracts the humidity of the atmoſphere, will ſoon fill the inſide of the piſtol with a coat of moiſture, and prevent the electrical ſpark from kindling the inflammable air.

It appeared, that a little camphire diſſolved in æther increaſes its exploſive force, and makes it leſs apt to fail.

As this inflammable air is heavier than common air, it is clear, that the mouth of the air piſtol ſhould be kept upwards at the time of charging it; whereas it is better to invert the piſtol when the ordinary inflammable airs are employed, which, being ſpecifically lighter than common air, riſe of themſelves in the piſtol when its mouth is placed inverted upon the orifice of the veſſel which contains them.

It is true, that the ſqueezing the elaſtic gum bottle, when placed upon the piſtol, forces ſome of the inflammable gaſs out of it, which is loſt in the common air; but notwithſtanding this waſte, the inflammable air which

remains in the piſtol is ſufficient to produce a loud report, which is all that is required. Indeed, one ſingle drop of the æther could be eaſily ſhaken out of the glaſs tube immediately into the piſtol, without making uſe of the elaſtic gum bottle; but this drop, evaporating into elaſtic air, leaves behind it a good deal of moiſture, whether inherent in the æther itſelf, or attracted from the atmoſphere. This moiſture, in the way I uſe to load the piſtol, remains in the elaſtic gum bottle, which is therefore always found moiſt when the experiment is repeated ſeveral times.

It was, indeed, known before this time, that æther and other volatile inflammable liquors ſpread by evaporating inflammable effluvia through the ſurrounding air, eſpecially when they are heated; and that theſe effluvia have ſometimes by the imprudent approach of a candle taken fire, and conveyed the inflammation to the liquor itſelf: but I never heard that any body employed theſe liquors inſtead of ordinary inflammable air in communicating to common air an exploſive quality, or in firing inflammable air piſtols, before I communicated the experiment to my friends

As it will, I think, appear very probable, by what will be ſaid hereafter, that little more than a pleaſing amuſement can be expected from the force of any inflammable air;

air; the ready production of such inflammable air always ready for use, when an explosion is intended to be produced, may be of some importance to philosophers whose time must be sparingly taken up.

Comparative view of the expanding force of explosive air and gunpowder.

The force of gunpowder has been ascribed by Sir ISAAC NEWTON and all other philosophers to the sudden extrication of an amazing quantity of elastic permanent aerial fluid within a narrow space incapable of containing it; which quantity ought to be attentively attended to, in order to estimate the comparative power of the two substances.

BENJAMIN ROBINS, whose work, intitled, *New Principles of Gunnery*, passes in this country for a standard book, affirms, that gunpowder, when fired, generates a permanent elastic fluid of 250 times the bulk of the powder before it was fired. He found, that common air, which is heated by the contact of a red-hot iron, expands to four times its former bulk. Hence he concludes, that the elastic air, disengaged from gunpowder, must expand also to four times its dimension; and that it occupies about a thousand times the bulk of the powder in the moment of inflammation.

The learned Count SALUCE remarks (*b*), that the elaſtic air generated by the inflammation of gunpowder occupies, when cold, 222 times the bulk of the powder, which agrees, as he finds, with the computation made by Meſſ. HAUKSBEE, AMONTONS, and BELIDOR. This calculation he confirms in the *Melanges de Philoſophie et des Mathematiques de la Societé Royale de* Turin, which is a continuation of the former work. He is of opinion, that this elaſtic fluid is of the ſame nature with common air (which was likewiſe the opinion of Dr. HALES); and that the prodigious force of gunpowder depends upon the action of the fire on all its parts, by which this fluid exerciſes all the force of its elaſticity.

The extrication of ſuch a conſiderable quantity of a permanent elaſtic fluid by the firing of gunpowder has been put to a particular uſe by ſeveral philoſophers.

The celebrated Mr. DE LA CONDAMINE gives an account of a braſs air gun contrived by one Mr. MATY, of Turin; which he loaded with air condenſed by firing in it two ounces of gunpowder, which, by its inflammation, let looſe ſuch a quantity of air as was ſufficient to ſhoot a leaden bullet ſixty paces, and to repeat the proceſs

(*b*) Miſcellanea Philoſophico-Mathematica Societatis privatæ Taurinenſis, p. 125.

eighteen

eighteen times, the ſtrength of the exploſion diminiſhing gradually as in other air guns [d].

In the learned work of Mr. ANTONI, an Italian gentleman, I find the ſame experiment, the account of which is accompanied with a figure of ſuch an air gun, in which the author fired one ounce of gunpowder, the barrel being very ſtout, and of a ſize capable of containing ten ounces. He afterwards let a quantity of this compreſſed air out by a valve in the ſame way as it is done in the common air guns. This one ounce of gunpowder yielded air enough to propel a leaden bullet through a board three lines thick, at the diſtance of forty paces, and to repeat the proceſs ſixteen or eighteen times [e].

The difference of the quantity of elaſtic fluid obtained from the firing of gunpowder by Mr. ROBINS and others might be owing to the difficulties attending the inveſtigation, or to the different proportion of the ingredients uſed in the compoſition of the powder; as it is well known, that gunpowder for the uſe of the army is made of five or ſix parts of nitre to one of charcoal,

(*d*) Extrait d'un Journal de Voyage d'Italie, par M. DE LA CONDAMINE, inſéré dans les Memoires de l'Academie Royale des Sciences de Paris, 1757, p. 405.

(*e*) Examen de la Poudre traduit de l'Italien de M. ANTONI, par M. le Viſcomte de FLAVIGNY, 1773.

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and one of ſulphur; when ſeven parts of nitre are uſed, it is called *poudre d'artifice*.

The celebrated Mr. JOHN BERNOUILLI calculates the denſity of the air contained in a ſolid ſtate in gunpowder to be $\frac{1}{1000}$ of what this fluid is when it conſtitutes a part of our atmoſphere. But he does not conſider this air as exiſting in all the component ingredients of the gunpowder, but chiefly in the nitre: and Count SALUCE ſuppoſes, that that part of the gunpowder which contains this air conſtitutes a conſiderable part of its bulk (though ſomewhat leſs than the half). Let us now ſuppoſe, that part of the gunpowder which contains this air to be not much leſs than the half of the whole maſs (for it would be difficult to demonſtrate accurately to what proportion of the whole maſs this part amounts in reality). On this ſuppoſition we ſhall find, that the whole maſs of gunpowder contains a quantity of air in a ſolid ſtate which is reduced in bulk to near $\frac{1}{500}$, or, in other words, that one ſquare inch of gunpowder contains near 500 ſquare inches of air; which being heated in the moment of inflammation will expand to four times its diameter; ſo that according to this calculation gunpowder muſt expand in the moment of exploſion to near 2000 times its own bulk.

It ſeems very probable, that this calculation of Mr. BERNOUILLI is much nearer the truth than that of

Mr. ROBINS and Count SALUCE; but yet the evaluation of the two laſt mentioned writers, though ſhort of one half, proves in reality Mr. BERNOUILLI's calculations to be as juſt as can be expected, when it is conſidered, that this evaluation was made before the new diſcoveries upon the nature of nitre and charcoal. But this aſſertion will be better underſtood when I have explained the nature of gunpowder ſomewhat fuller.

If we continue to ſay, as we have hitherto done, that the charcoal taking fire decompoſes the nitre, and extricates from it that amazing quantity of elaſtic fluid which was ſhut up within its ſubſtance, we only ſay what we ſee in reality is the conſequence of ſetting fire to this ingredient. But this explanation does not convey a clear idea of the manner in which the extrication is carried on; nor of the reaſon why one ſingle ſpark of fire, thrown into an immenſe heap of gunpowder, ſhould almoſt in an inſtant ſpread the conflagration through the whole maſs. Neither does it explain clearly, why nitre and charcoal (which ſeparately yield no flame at all, though ever ſo much heated) when combined and intimately mixed together, explode with as loud a report as a large ordnance piece, ſurpaſſing even in loudneſs thunder-claps; nor why this forcible exploſion is accompanied by a moſt brilliant flame.

Nitre

Nitre is compoſed of two different ingredients, *viz.* an acid, called from its peculiar nature the *nitrous acid*, and the vegetable alkali. Neither of theſe two ingredients are capable of inflammation; nay, they even extinguiſh actual fire. When they are both combined and conſtitute the neutral ſalt we ſpeak of, they have not, even by their coalition in one body, acquired an inflammable quality, for nitre may be made red-hot in a crucible without ſhewing the leaſt appearance of inflammation, not even when a red-hot ſtone or piece of iron is thrown into it. But if any common combuſtible ſubſtance, as wood, charcoal, or ſuch like, is thrown into the melted nitre, a flame iſſues with a kind of exploſion, though only at the very place where the two ſubſtances come into contact. The ſame flame and exploſion is obſerved when cold nitre is thrown upon a combuſtible body, in a ſtate of real ignition, on a piece of red-hot charcoal for inſtance.

The true reaſon of this wonderful phenomenon has not been conſidered hitherto with that degree of attention it deſerves, and could not have occurred to any body before our modern philoſophers had diſcovered the nature of various kinds of air, and the manner of extracting them from bodies.

Inflam-

Inflammable air, the *gas flammeum* of VAN HELMONT, was conſidered as an aerial fluid, ſuſceptible of inflammation by itſelf.

But we now know, that inflammable gas concentrated will not burn at all; but on the contrary extinguiſhes flame. Mr. CAVENDISH was the firſt who ſet this matter in a proper light. He diſcovered, by experiments, that a mixture of a ſmall quantity of common air with a great proportion of inflammable air, as, for inſtance, two parts of common air with eight parts of inflammable air, caught fire without noiſe, and conſumed gradually; but that three parts of inflammable air and ſeven parts of reſpirable air exploded with a very loud ſound.

It was Dr. PRIESTLEY's important diſcovery which ſuggeſted to me the theory I intend to lay before the Royal Society.

This acute philoſopher found, that, if inſtead of common air, dephlogiſticated air is mixed with a due proportion of inflammable air, the exploſion is conſiderably louder.

The principal ingredients of gunpowder, and thoſe to which it owes its force, are nitre and charcoal; for theſe two ingredients, well mixed together, conſtitute gunpowder at leaſt equal if not ſuperior in ſtrength to common gunpowder (as I found by experience, and may be

ſeen in the Memoire of Count SALUCE, inſerted in the *Melanges de Philoſophie et de Mathematiques de l'Acad. Royale de* Turin). The ſulphur ſeems to ſerve only for the purpoſe of ſetting fire to the maſs with a leſs degree of heat.

Nitre yields by heat a ſurprizing quantity of pure dephlogiſticated air. Charcoal yields by heat a conſiderable quantity of inflammable air. The fire employed to inflame the gunpowder extricates theſe two airs, and ſets fire to them at the ſame inſtant of their extrication. Thus the difference between the inflammation of gunpowder and that of a mixture of inflammable air with dephlogiſticated air in an ordinary air piſtol (as theſe laſt are now contrived for a philoſophical amuſement) ſeems to be, that the compound of the two airs in the air piſtol takes fire, when already extricated and exiſting in a ſpace without compreſſion or condenſation; that is to ſay, when they are in no condition of exerting a much greater elaſticity than what they acquired by the heat generated in the moment of their exploſion; which heat can only expand them to four times their former bulk, according to Mr. ROBINS: whereas in gunpowder the two airs, exiſting in a ſolid ſtate before their extrication, and occupying, according to Mr. ROBINS, about $\frac{1}{250}$ of the ſpace they take up after they are ſet looſe, but moſt probably

bably even less than $\frac{1}{500}$ (as will be seen by and by), are extricated all at once, when confined (as in fire arms) in a space $\frac{1}{150}$ or rather $\frac{1}{500}$ times less than they can occupy when reduced to the temperature of the common atmosphere, and of consequence 2000 less than they can occupy when heated in the moment of inflammation; so that the difference of the explosive force in the inflammation of the two compounds can be no less than as 4 is to 2000.

It must be here remembered, that air being a very compressible body, a moderate resistance acting against its rarefaction easily overcomes the force of its expansion, when this expansion or rarefaction does not amount to more than four times its bulk (that such a power ought not to be very great, we know by the force employed in ordinary wind guns and condensing machines) whereas no condensing machine has yet been contrived by which air could be condensed to any thing approaching the state of condensation of this fluid as it exists in the substance of nitre.

It might be here objected, that air compressed to one tenth in a wind gun possesses a power not much short of gunpowder, though only $\frac{1}{18}$ or $\frac{1}{20}$ of it is let loose at a time; and that thus, inflammable air, though expanded only four times in the moment of inflammation, may exert a force approaching that of the wind gun, the whole

maſs of the charge being employed in one and the ſame explosion. This compariſon is very inadequate; for in the caſe of a wind gun the air compreſſed to $\frac{1}{10}$ is ready to exert all the force of elaſticity exiſting in the whole maſs, and may therefore be compared to a ſtrong ſpring forcibly bent. But the inflammable air is far from exerting the force of expanſion and elaſticity through its whole maſs at the ſame inſtant: for the inflammation is propagated through it ſucceſſively, beginning where the electrical ſpark kindles it, and reaching gradually farther till the whole is conſumed. Now as I have demonſtrated, that inflammable air is reduced to more than half its bulk by inflammation, it muſt follow, that that portion of it which is conſumed the firſt by the inflammation, leaving more room by its diminution, diminiſhes in proportion the propelling powers of what remains ſtill to be inflamed.

The very great difference between the explosive force of the two compounds is illuſtrated by what happens after their inflammation. The compound of inflammable with common or dephlogiſticated air, is very much reduced in bulk after inflammation. I found this by the following experiment: I fired a braſs inflammable air piſtol (made by Mr. NAIRNE according to my directions) which had a piſton in the cylinder, by which a proper quantity

quantity of reſpirable and inflammable air was drawn in. I had rammed into the barrel, adapted to it, a leaden bullet wrapped up in a piece of leather ſo ſtrongly that I did not expect the reſiſtance could be overcome by the exploſion. I fired it by an electrical ſpark; the inflammation took place, the piſtol grew hot, the ball was not propelled, and the piſton was driven more than half way down the cylinder by the preſſure of the atmoſphere acting upon it when the exploſive air was conſumed by the inflammation.

The caſe is quite different in the firing of gunpowder, as there remains after its inflammation a maſs of air which occupies about 250 times the former bulk, according to Mr. ROBINS.

As, in the foregoing experiment, the compound of inflammable and common air was reduced above the half of its former bulk, it ſeems more than probable, that the quantity of dephlogiſticated and inflammable air extricated in the firing of gunpowder muſt alſo undergo a ſimilar diminution by its inflammation; ſo, that when there remains a maſs of air, 250 times the bulk of the gunpowder, the quantity of air extricated from the powder muſt have been in reality not leſs than 500 times the bulk of the powder, which agrees nearly with the calculation of Mr. JOHN BERNOUILLI. Let us now

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ſee how far this computation agrees with the analyſis of gunpowder. Abbé FONTANA, ſo advantageouſly known by his important diſcoveries in natural philoſophy, more eſpecially by thoſe he has made on the various kinds of air, favoured me with the following reſult of his experiments. An ounce of nitre, expoſed to a great degree of heat for the purpoſe of extracting its air in the uſual way, yielded about 800 cubic inches of dephlogiſticated air. An ounce of charcoal, treated in the ſame way, gave about 150 cubic inches of air, partly fixed, partly inflammable, mixed with ſome common air.

Let us now calculate (without, however, being too ſcrupulous about the accuracy of the reſult) what quantity of elaſtic permanent fluid a cubic inch of ſolid gunpowder will give in the moment of deflagration: a cubic inch of ſolid gunpowder contains in weight 442 grains (which is 38 grains ſhort of an ounce Troy weight) of which 331$\frac{1}{2}$ grains is nitre, 55$\frac{1}{4}$ charcoal, and as much ſulphur (ſuppoſing the proportion of the ingredients of the powder to be ſix parts of nitre to one of charcoal and one of ſulphur); 331$\frac{1}{2}$ grains of nitre will give about 552 cubic inches of dephlogiſticated air; 55$\frac{1}{4}$ grains of charcoal will produce about 17 cubic inches of air, chiefly inflammable, according to the calculation of Abbé FONTANA.

By

By this calculation, which will, perhaps, be found more accurate than the former, one cubic inch of ſolid gunpowder will yield above 569 cubic inches of permanent elaſtic fluid: I ſay, above 569 cubic inches, for I do not put into the account the elaſtic fluid which is generated by the ſulphur, nor that which charcoal, conſumed by the inflammation of the gunpowder, yields above the quantity mentioned, which it gives when heated in a glaſs veſſel, by which it is by no means conſumed, an ounce loſing by this operation only 60 grains of its weight.

As this elaſtic fluid will increaſe to four times its bulk, it follows, that one cubic inch of ſolid gunpowder will extricate in the moment of exploſion above 2276 cubic inches of elaſtic air. Which computation is not far from the reſult of my former calculation, and that of Mr. BERNOUILLI.

An accurate calculation of the expanſion of gunpowder would be a very difficult undertaking. The expanſion of the moiſture always contained in gunpowder, however dry, may alſo contribute its ſhare towards the amazing powers of this ingredient. Nitre contains from its nature a great ſhare of water, which is neceſſary for the cryſtallization of it, and charcoal is always found to contain it. We know,

that very hot vapour is capable of occupying almoſt 2000 times the ſpace it did in the ſtate of cold water.

The generation of dephlogiſticated and inflammable air by the inflammation of gunpowder is the reaſon why this ingredient is almoſt the only one known, which does not want a free acceſs of common air to be conſumed by fire; and therefore it may be ſaid to feed, as it were, upon its own air.

This theory of gunpowder induces me to venture a new one of the *pulvis fulminans*, which conſiſts of three parts of nitre, of two of fixed alkaline ſalt, and of one of ſulphur. This powder much ſurpaſſes the force of gunpowder in exploding, with a very loud report, in the open air when it is heated to a certain degree. It is commonly ſaid, that in the heating of this powder the ſulphur joins with the alkaline ſalt and conſtitutes an *hepar ſulphuris*, which riſing up in bubbles confines the air contained in them, which air at laſt becomes ſo powerfully expanded that it overcomes and breaks through the reſiſtance of the coercive bubbles of the *hepar ſulphuris*, with all the force of its elaſticity; which ſudden emerſion muſt naturally occaſion a proportional ſound. But I think, that the nitre contained in this powder, being heated, yields its dephlogiſticated air when the melting ſulphur yields inflammable air; at the ſame time the ſulphur conſtitutes

with

with the alkaline salt, an *hepar sulphuris*, which rising in tough bubbles confines this explosive air generated. At length, however, the increasing heat, which sets fire to the sulphur, sets this explosive air on fire also; which then following its own nature explodes with so much the more force from its having been entangled and confined within the bubbles of the *hepar sulphuris*.

After what has already been said, it will not be difficult to explain, why a single spark of fire propagates the combustion with great rapidity through the whole mass of gunpowder, however great. If we put a single grain of gunpowder upon a red-hot iron, we see the particles of red-hot charcoal projected with great rapidity in every direction by the forcible explosion of the two airs extricated in the manner before explained. Thus, if one or more grains, among a heap of others, are set fire to, the particles of red-hot charcoal being driven with great violence against the surrounding grains communicate their heat to all the particles of charcoal they hit, which particles, by heating the particles of nitre in close contact with them, extricate their dephlogisticated air at the same time that the charcoal yields its inflammable air; in consequence of which a more powerful explosion happens. This secondary explosion projects with a much greater force the particles of charcoal surrounded by the explosive

flame of the two airs; and thus the conflagration ſpreads with a very great velocity through the whole maſs, though always by ſucceſſion. The quickneſs of this propagation of fire depends in a great meaſure upon the intervals or interſtices which remain among the grains of gunpowder, through which the particles of heated charcoal are driven in every direction, together with the flame of the two airs. Hence gunpowder reduced into impalpable powder, and rammed into a ſquib, does not inflame with an exploſion, but burns ſlowly farther and farther till the combuſtion reaches the extremity of the ſquib, where it meets a maſs of gunpowder in grains, when immediately a loud exploſion iſſues, by which the ſquib is ſhattered into rags. Hence the ſize of the grains of gunpowder muſt be proportionate to the ſize of the fire arms to which it is deſtined, the greateſt fire arms requiring in general grains of the largeſt ſize.

If this wonderful and awful ingredient had not been diſcovered by accident, could the ſecret have eſcaped a long while the penetration of our modern philoſophers, who have found out the way of combining the air of the two conſtituents after they had extricated them, without any regard to the known properties of gunpowder? Nothing more was to be done than combining the two ſub-

ſtances inſtead of combining the two airs firſt ſeparated from them.

APPENDIX.

IN the foregoing paper I attempted to give a comparative view of the explosive force of gunpowder and inflammable explosive air, which latter I had found to be ſo far ſhort of the explosive force of gunpowder as not to conceive any well grounded hope that it could ever become a ſubſtitute to this ingredient.

At that time I had not yet tried the effect of very pure dephlogiſticated air combined with that inflammable air, into which I had found that vitriolic æther is changed in an inſtant.

I muſt acknowledge, that I had but ſmall expectations from the force of theſe two airs combined; for as I had always obſerved, that æther air combined with common air is leſs briſk in taking fire, and leſs powerful in exploding, than inflammable air extracted from the vitriolic or marine acid, I thought that the ſame æther air combined with very pure dephlogiſticated air would alſo

be leſs powerful than common inflammable air from metals. But how far experience contradicted this theoretical analogy will be ſeen in the following lines.

Abbé FONTANA was ſo good as to aſſiſt me in this purſuit. Having produced a good quantity of pure dephlogiſticated air from red precipitate by heat, we firſt filled a ſtrong two-ounce phial (the orifice of which was ſo wide that it could ſcarce be covered with the thumb, ſo that the bottle was almoſt cylindrical) with this air, in the uſual manner, by filling it firſt with water, inverting it, and letting the air riſe in it; which being done, we dropped one drop of æther (in which a ſmall quantity of camphire was diſſolved) into it, and ſhut it immediately with the thumb. After having given it ſome concuſſions, the orifice was applied to the flame of a candle, by withdrawing the thumb when the orifice was cloſe to the flame: the air inſtantly took fire, and exploded with ſuch a ſtrong report, that, if the phial had not been very ſtout, it would moſt probably have been ſhattered into pieces, notwithſtanding its wide orifice. We repeated the ſame experiment with the ſame ſucceſs.

I was the more aſtoniſhed at the uncommon loud report (conſidering the wide orifice of the phial), becauſe, having often tried æther air in the ſame way with common air, I never found it explode with any con-

conſiderable degree of force; and therefore I found it neceſſary, in order to procure a loud report, to kindle it by an electrical ſpark directed through the piſtol, when its orifice was ſhut up by a cork, the reſiſtance of which was the chief cauſe of the report.

This wonderful effect in an open veſſel could not fail of giving me a good expectation of a very powerful effect, if this compound air was ſhut up in an air piſtol by a cork ſqueezed into its orifice. As it had been now kindled twice by the flame of a candle, I wanted to kindle it by the ſame means in an air piſtol; for this purpoſe we drilled a ſmall hole in the ſide of the piſtol, which was made of tin, and contained about nine cubic inches of ſpace. We filled it with dephlogiſticated air in the ſame manner as we had filled the phial by means of water; and after having poured into it one drop of æther by means of a glaſs tube (in the manner above deſcribed), we ſhut the orifice by thruſting a cork into it, and kept a finger applied to the touch-hole which was drilled in the ſide of the piſtol. To avoid accidents if the piſtol ſhould burſt, we thought it prudent to ſqueeze the cork very gently into the orifice, ſo that the reſiſtance ſhould be very moderate. Abbé FONTANA wrapped a towel round the piſtol for ſecurity's ſake, leaving only the touch-hole uncovered; which being brought near the flame

of

of a wax taper, the air inſtantly took fire, and exploded with ſuch a ſtrong report, that his hearing, as well as mine, was much hurt by it. The cork, which was a very ſound one, flew to pieces againſt the wall; and the Abbé felt ſuch a conſiderable ſhock in his hands, that he did not think it ſafe to repeat the experiment, unleſs a ſtronger piſtol could be procured.

Encouraged by ſuch uncommon and unexpected effects, I went immediately to Mr. NAIRNE to inquire, whether he ſtill had in his poſſeſſion a ſtrong braſs air piſtol, which he had made laſt ſummer according to my direction? I was lucky enough to find it: nothing was to be done to it but to drill a touch-hole in the left ſide of it, in order to kindle it by a flame if required. This touch-hole was to be ſhut up by a braſs male ſcrew fitted exactly to it, when the piſtol was intended to be fired by an electrical ſpark.

The air box of this piſtol was a cylinder four inches long and two inches in diameter. The fore part of the air box to which the piſtol barrel fitted to receive a leaden ball or a cork, was fixed, had a broad ſhoulder, which was faſtened to the body of the air box by ſix ſtrong braſs ſcrews, which never had been looſened by former exploſions. A leaden bullet, wrapped up in leather, was forcibly rammed into the piſtol barrel as far as the ſcrew, which

joins

joins the barrel with the air box (as may be ſeen in the figure). The piſtol was filled with pure dephlogiſticated air (which was drawn-in by the piſton from an elaſtic gum bottle), and one drop of æther being poured into it, the air within was kindled by an electrical ſpark directed through it. The air took fire: the exploſion was as loud as that of a common muſket, and the force ſo great, that the whole fore part of the air box with the piſtol barrel flew off, all the ſix ſcrews were broke, and the ſtrong and tough metal of which they were made was rent. Three ſtrong braſs ſcrews, by which the bottom of the air box was fixed to the wooden handle, were looſened, and the whole frame of the piſtol was out of order. The ſubſtance of the air barrel, where it was tore, was of the thickneſs of about a half crown piece.

Being now convinced, that though inflammable air from metals with dephlogiſticated or common air, is far inferior to the force of gunpowder, the exploſive force of the compound of dephlogiſticated and æther air approaches it much nearer, I thought it worth while to fit the piſtol up in ſuch a manner as to be out of all danger of burſting. For this purpoſe I deſired Mr. NAIRNE to adapt, and ſolder to the fore part of the air box, a hollow cone of braſs, the extremity of which ſhould terminate in the gun barrel.

As

As the piſton could not reach to the extremity of this conical hollow (which conſequently muſt be always filled with common air), I deſired him to fix to the piſton an ivory cone, through which the two wires would paſs to meet one another at the ſurface of the cone, leaving an interſtice between them of about one line, through which the electrical ſpark ſhould leap and ſet fire to the air. This ivory cone ſhutting up exactly the whole cavity of the air box, no air could come into it but what was drawn in by the piſton.

The piſtol thus fitted up anſwered tolerably well. The figure joined to it will ſerve to give a better idea of the whole contrivance than could be well explained by words. The ſcale is in the proportion of one third of the real ſize.

The cone, inſtead of ivory, may be made of ſolid glaſs, which is a better non-conductor than ivory. The canals in the ivory, through which the two wires paſs, may be made wide enough to contain a glaſs tube, through which the wires paſs; or to be filled with a non-conducting cement, as ſealing wax, for the ſame purpoſe. The cone may even be made of braſs, provided two glaſs tubes are lodged in it, to give a paſſage to the two wires.

I kindle this piſtol ſometimes by putting in the touch-hole a little bit of a cotton thread ſoaked in moiſt gunpowder

powder and dried afterwards; or a bit of thoſe paper matches which the Chineſe put into thoſe little ſquibs, which go by the name of *India crackers*. I ſometimes kindle it by holding the flame of a candle or a burning paper to the touch-hole. In this caſe it is to be obſerved, that the touch-hole muſt be kept upwards, if the piſtol is loaded with inflammable air from metals, becauſe this air being lighter than common air, will riſe out of the hole and meet the flame. The contrary muſt be done when æther air is employed, it being heavier than common air, and thus diſpoſed to deſcend and fall upon the flame kept under it.

To fill this piſtol with any air, I commonly firſt fill an elaſtic gum bottle with it, the orifice of which is juſt big enough to receive that part of the gun barrel which is fixed to the air box: thus, by ſqueezing between my feet the elaſtic gum bottle, I draw in at the ſame time the air by drawing up the piſton. A bladder is alſo very fit for this purpoſe, and has the advantage above an elaſtic gum bottle in not requiring to be ſqueezed to draw the air out of it.

Inflammable air from metals will riſe in the piſtol of itſelf, when its orifice is kept upon the bottle containing it.

If the piſtol is deſtined to be always kindled by the flame of a candle or a match, as I have deſcribed, it would be better to have no piſton to it, as it may then be filled by the means of water, and the exploſive force will be ſo much the greater, as ſome of the flame makes eaſily its way over the leather of the piſton, and ruſhes out backward, which, I find, is often the caſe, if the bullet is rammed in the barrel ſomewhat too tightly.

It would, perhaps, not be an eaſy undertaking to give a ſatisfactory reaſon, why a drop of æther communicates to dephlogiſticated air a much ſtronger exploſive force than common inflammable air from metals. May it not be ſaid, that common inflammable air from metals, having only about one fifth of the ſpecific gravity of the dephlogiſticated air, the two fluids do not penetrate one another ſo readily and ſo intimately as the compound of dephlogiſticated and æther air, which are both nearly of the ſame ſpecific gravity, each being ſomewhat heavier than common air? for it ſeems not improbable, that the ſwiftneſs with which the flame is propagated through the maſs of this compound air, depends partly on the intimate mixture of the phlogiſton with the dephlogiſticated air. Might not this phenomenon be aſcribed to the greater bulk of inflammable air from metals compared with the ſmall compaſs which one ſingle drop

drop of æther occupies, which last ingredient, when pure, seems to be an essence of the inflammable principle of the spirit of wine, a pure phlogiston concentrated in the form of a liquid? Indeed the inflammable air from metals seems to be rather a compound of phlogiston and some kind of elastic permanent fluid than a pure inflammable fluid; for this air, after having lost all its inflammability, by being kept a long while upon water, occupies still a considerable space, and is then become phlogisticated air; that is to say, such an air as is not to be diminished by nitrous air, or to be inflamed.

Though I have no reason to alter my former assertion, that the force of gunpowder is proportionable to the sudden extrication of a great quantity of the elastic fluid generated in the moment of conflagration, and the expansion of this fluid by heat, communicated to it in the same moment of its extrication; and that the force of inflammable explosive air can only be proportionable to the sudden expansion by heat in the moment of the inflammation (for no new extrication here takes place); yet I did not consider enough in the account the suddenness of this expansion, which may make a considerable difference in the force of the explosion. And indeed the abovementioned experiments seem to demonstrate, that the inflammation of the compound of pure dephlo-

gisticated and æther air spreads with such a velocity through the whole mass as to be almost instantaneous.

It is well known, that mechanical power chiefly depends upon the velocity with which a body is endowed in the instant of exerting it; or that the *momentum*, or force of a body, must be computed by multiplying the quantity of matter into the velocity with which it moves. Thus, if this new compound of dephlogisticated and æther air expands with ten times greater velocity than any other inflammable explosive air, its force will be about ten times greater.

As it seems to be probable, from what is already said that this compound of explosive air may be put to more uses than that of an amusing experiment, I think it worth while for men engaged in this branch of natural philosophy to look out for a method of producing at pleasure any quantity of dephlogisticated air required. Considering the rapid progress which is daily made on the important subject of air, I cannot but flatter myself, that this great discovery is not far off. The benefit which would arise from such a discovery for animal life must encourage every philosopher to pursue this object. Indeed, if we consider that nitre contains this wonderful aerial fluid in a most concentrated state, and that the nitrous acid seems to be nothing else but this beneficial

fluid

fluid combined with phlogiſton, which ſeems to be imbibed by the vegetable alkali, when the acid is expelled by heat in the form of this air; that this beneficial aerial fluid exiſts alſo, in a moſt concentrated ſtate, in bodies almoſt every where to be found, as are calces of metal, principally that of iron; that common water contains it in great abundance, ſo that the light and warmth of the ſun extracts it to one fifteenth of the bulk of the water, as Dr. PRIESTLEY found, that even the maſs of our atmoſphere is nothing elſe but this very air ſoiled with impurities. If we conſider, I ſay, all this, is it not reaſonable to hope, that we are near the important inſtant when this ſalubrious aerial fluid will be procured for many uſeful purpoſes in a ſufficient quantity, either by the diſcovery of a ready way to let looſe this air from the bodies in which it is as it were impriſoned, or by filtrating or purifying common air from its impurities?

EXPLANATION OF THE FIGURE.

A *a* the piſtol barrel.

B the large cylinder or air box.

b the place where the piſtol barrel unſcrews from the air box.

C the piſtol handle.

D the

D the handle fixed to the piſton at *b b*, which is ſquare to prevent the piſtons turning round.

E the hole in the ſide of the air box.

e the ſcrew to ſtop the ſaid hole.

F a piece of braſs with a female ſcrew, which was fixed with three ſtrong ſcrews to the wooden handle.

f a ſcrew at the end of the air box which ſcrewed into the piece F.

G a piece of ivory fixed to the piſton.

K the piſton, with the piece of conical ivory fixed to it.

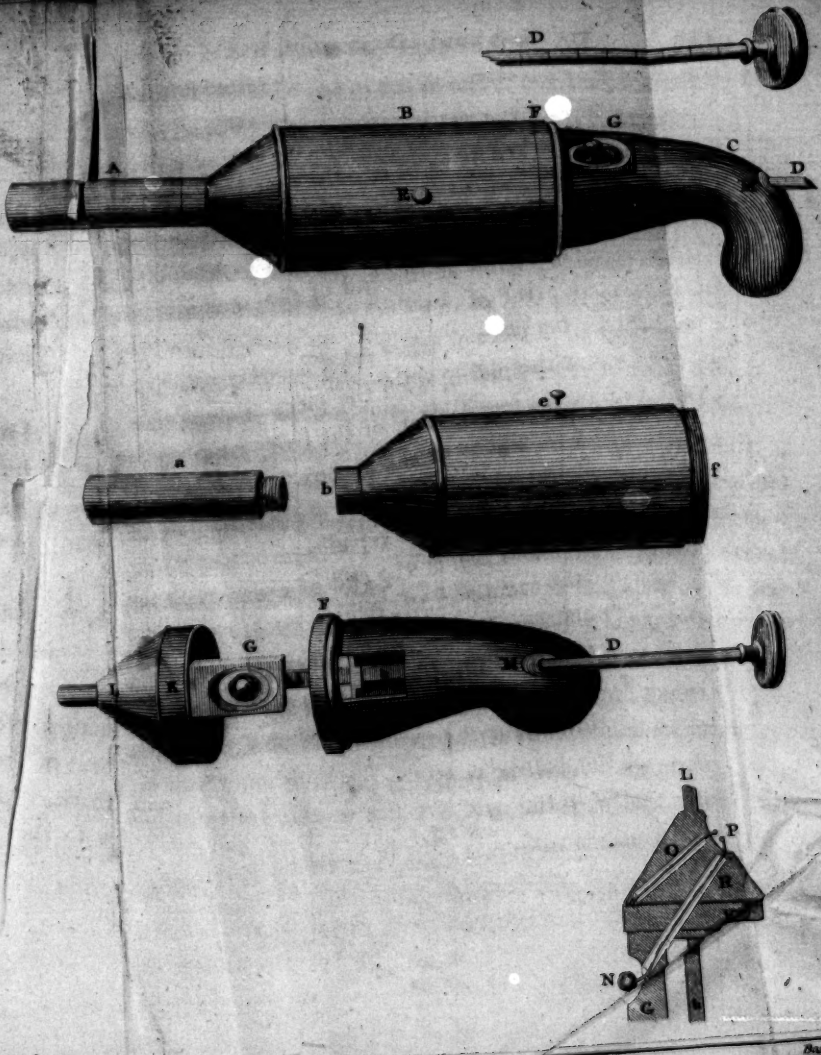
L the termination of the ivory cone which filled up the ſmall end of the air box.

M the perforation in the wooden handle, through which the braſs handle D paſſes.

N a braſs ball at the end of a wire, which paſſes down the canal R in the ivory cone.

O another piece of wire paſſing down the other canal, canals are filled with non-conducting ſubſtance.

P an interſtice between the two wires of about one line, through which the electrical ſpark, when given on the ball N, paſſes and ſets fire to the inflammable air in the air box.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XXVII. *The Description of Two new Micrometers. By Mr.* Ramsden, *Optician; communicated by* Joseph Banks, *Esq. P. R. S.*

Read March 25, 1779.

WHEN I offer the description of a new micrometer to the learned members of the Royal Society, I do not flatter myself that mere novelty will entitle it to their patronage. Sensible how much the theory of astronomy is limited by the imperfection of instruments, I always incline to improve rather than invent, except when repeated examinations convince me, that the imperfections arise from defect in principle as well as in the construction.

In considering micrometers, when an observer finds, that with the micrometer, which depends on moveable parallel wires, he cannot measure any diameter of a planet, except that which is at right angles to the direction of its apparent motion, he cannot withhold his preference to that construction which measures the angle by the separation of the images. It appeared therefore to

to me a matter of ſome importance, to inveſtigate the cauſes of the uncertainty which has been found in the obſervations made with the micrometer with a divided object-glaſs. The reſult of my examination convinced me, that were it poſſible to execute the conſtruction of that micrometer, with the degree of accuracy required, it muſt ſtill be ſubject to inaccuracy from its principle.

By the poſition of the micrometer every error of its glaſs is magnified by the teleſcope; and if each ſurface of the micrometer glaſs has not, in every part, preciſely the ſame radius (which opticians muſt allow to be exceedingly difficult) there will be a conſiderable error in the angle to be meaſured, and the eye applied to the different parts of the pencil will, without moving the micrometer, ſee the images of the object in the teleſcope fluctuating, ſometimes appearing to overlap, and ſometimes to ſeparate from each other.

But ſuppoſing the glaſs itſelf to be perfect in its ſubſtance and in its curvature, there will yet remain imperfections which ariſe from its principle. A micrometer glaſs applied to a teleſcope cauſes a very conſiderable abberration. If the focus of the glaſs is poſitive, the extreme aberration will be within the geometrical focus; if negative, it will be beyond it: and the aberration not only affects

affects the diſtinctneſs of the image, but alſo the angle meaſured by the micrometer.

At the time I took up this ſubject, the divided object-glaſs micrometer was the only one which meaſured angles by the ſeparation of two images. Since that time, a very ingenious application of the priſm to this purpoſe has been invented by the rev. Dr. MASKELYNE, Aſtronomer Royal; and although experience has not yet aſcertained the extent of its merit, it will always deſerve great conſideration from its ingenuity; but the more I conſidered the ſubject, I became more fully convinced, that the principle of reflection applied to micrometers would have great advantages over thoſe hitherto conſtructed on the principle of refraction; and the catoptric micrometer I have the honour to deſcribe, beſides the advantage it derives from the principle of reflection, of not being diſturbed by the heterogeniety of light, avoids every defect of other micrometers, and can have no aberration, nor any defect which ariſes from the imperfection of materials, or of execution, as the extreme ſimplicity of its conſtruction requires no additional mirrors or glaſſes to thoſe required for the teleſcope: and the ſeparation of the image being effected by the inclination of the two ſpecula, and not depending on the focus of any lens or mir-

ror, any alteration in the eye of an obſerver cannot affect the angle meaſured.

It has, peculiar to itſelf, the advantages of an adjuſtment to make the images coincide in a direction perpendicular to that of their motion; and alſo of meaſuring the diameter of a planet on both ſides the zero, which will appear no inconſiderable advantage to obſervers who know how much eaſier it is to aſcertain the contact of the external edges of two images than their perfect coincidence. A ſhort explanation of the annexed drawings will make the conſtruction and the properties of this micrometer obvious.

I divided the ſmall ſpeculum of a reflecting teleſcope, of CASSEGRAIN's conſtruction, into two equal parts, by a plane acroſs its center; and by inclining the halves of the ſpeculum to each other on an axis at right angles to the plane that ſeparated them, I obtained two diſtinct images. The ſatisfaction I received on the firſt trial was checked by the apparent impoſſibility of reducing this principle to practice. The angular ſeparation of the two images in this caſe being half the angular inclination of the two ſpecula, it required an index of an unmanageable length, to allow the quantity of one ſecond of a degree to become viſible. Some time afterwards, on reviſing the principle, I conſidered, that if both the halves of the mir-

ror turned on their center of curvature, there could be no alteration in their relative inclination to each other from their motion on this center; and that any extent of ſcale might be obtained, by fixing the center of motion at a proportional diſtance from the common center of curvature. This will be better underſtood from the annexed drawing.

A (fig. 1.) repreſents the ſmall ſpeculum divided into two equal parts; one of which is fixed on the end of the arm B; the other end of the arm is fixed on a ſteel axis X, which croſſes the end of the teleſcope C. The other half of the mirror A is fixed on the arm D, which arm at the other end terminates in a ſocket *y*, that turns on the axis, X; both arms are prevented bending by the braces *a a*. G repreſents a double ſcrew, having one part *e* cut into double the number of threads in an inch to that of the part *g*: the part *e* having 100 threads in one inch, and the part *g* 50 only. The ſcrew *e* works in a nut F in the ſide of the teleſcope, while the part *g* turns in a nut H, which is attached to the arm B; the ends of the arms B and D, to which the mirrors are fixed, are ſeparated from each other by the point of the double ſcrew preſſing againſt the ſtud *b*, fixed to the arm D, and turning in the nut H on the arm B. The two arms B and D are preſſed againſt the direction of the double ſcrew *eg* by

a ſpiral ſpring within the part *n*, by which means all ſhake or play in the nut H, on which the meaſure depends, is intirely prevented.

From the difference of the threads on the ſcrew at *e* and *g* it is evident, that the progreſſive motion of the ſcrew through the nut will be half the diſtance of the ſeparation of the two halves of the mirror, and conſequently the half mirrors will be moved equally in contrary directions from the axis of the teleſcope C.

The wheel V fixed on the end of the double ſcrew has its circumference divided into 100 equal parts, and numbered at every fifth diviſion with 5, 10, &c. to 100, and the index I ſhews the motion of the ſcrew with the wheel round its axis, while the number of revolutions of the ſcrew is ſhewn by the diviſions on the ſame index. The ſteel ſcrew R may be turned by the key S, and ſerves to incline the ſmall mirror at right angles to the direction of its motion. By turning the finger head T (fig. 2.) the eye tube P is brought nearer or farther from the ſmall mirror, to adjuſt the teleſcope to diſtinct viſion; and the teleſcope itſelf hath a motion round its axis for the conveniency of meaſuring the diameter of a planet in any direction. The inclination of the diameter meaſured with the horizon is ſhewn in degrees and minutes by a level and vernier on a graduated circle, at the breech of the teleſcope.

The

The method of adjuſting and uſing the catoptric micrometer is too obvious to require any explanation: it is only neceſſary to obſerve that, beſides the table for reducing the revolutions and parts of the ſcrew to minutes, ſeconds, &c. it may require a table for correcting a very ſmall error which ariſes from the excentric motion of the half mirrors. By this motion their centers of curvature will (when the angle to be meaſured is large) approach a little towards the large mirror; the equation for this purpoſe in ſmall angles is inſenſible, but when angles to be meaſured exceed ten minutes, it ſhould not be neglected. Or, the angle meaſured may be corrected by diminiſhing it in the proportion the verſed ſine of the angle meaſured, ſuppoſing the excentricity radius, bears to the focal length of the ſmall mirror.

The teleſcope to which the catoptric micrometer is applied is of the CASSEGRAIN conſtruction. The great ſpeculum is about twenty-two inches focus, and bears an aperture of $5\frac{1}{10}$ inches, which is conderably larger than thoſe of the ſame focal length are generally made: indeed, the apparent utility of this micrometer makes me wiſh to ſee the reflecting teleſcope meet with further improvements. I believe it would more tend to the advancement of the art of working mirrors, if writers on this ſubject, inſtead of giving us their methods of working ima-

imaginary parabolas, would demonſtrate the properties of curves for mirrors which, placed in a teleſcope, will ſhew images of objects perfectly free from aberration; or, what will yet be more uſeful in practice, of what forms ſpecula might be made, that the aberration cauſed by one mirror may be corrected by that of the other. If mathematicians aſſume *data* which really exiſt, they muſt ſee, that when the two ſpecula of a reflecting teleſcope are parabolas, they cauſe a very conſiderable aberration, which is negative, that is to ſay, the focus of the extreme rays is longer than thoſe of the middle ones. If the large ſpeculum is a parabola, the ſmall one ought to be an ellipſe; but when the ſmall ſpeculum is ſpherical, which is generally the caſe in practice, if concave, the figure of the large ſpeculum ought to be an hyperbola; if convex, the large ſpeculum ought to be an ellipſe, to free the teleſcope from aberration.

This will be eaſier underſtood by attending to the poſitions of the firſt and ſecond images; when a curve is of ſuch form that lines drawn from each image, and meeting in any part of the curve, make equal angles with the tangent to the curve at that point, it is evident, that ſuch curve will be free from aberration.

This is the property of a circle when the radiant and image are in the ſame place; but when they recede from

from each other, of an ellipſe, of ſuch form that the radiant and image are in the two *foci*, till one diſtance becoming infinite the ellipſe changes into a parabola, and to an hyperbola when the focus is negative; that is to ſay, when reflected rays diverge, and the focus is on the oppoſite ſide of the mirror.

Theſe principles made me prefer CASSEGRAIN's conſtruction of the reflecting teleſcope to either the Gregorian or Newtonian. In the former, errors cauſed by one ſpeculum are diminiſhed by thoſe in the other.

From a property of the reflecting teleſcope (which has not been attended to) that the apertures of the two ſpecula are to each other very nearly in the proportion of their focal lengths, it follows, that their aberrations will be to each other in the ſame proportion, and theſe aberrations are in the ſame direction, if the two ſpecula are both concave; or in contrary directions, if one ſpeculum is concave, and the other convex.

In the Gregorian conſtruction, both ſpecula being concave, the aberration at the ſecond image will be the ſum of the aberrations of the two mirrors; but in the CASSEGRAIN conſtruction, one mirror being concave, and the other convex, the aberration at the ſecond image will be the difference between their aberrations. By aſſuming ſuch proportions for the *foci* of the ſpecula as are gene-

rally uſed in the reflecting teleſcope, which is about as 1 to 4, the aberration in the CASSEGRAIN conſtruction will be to that in the GREGORIAN as 3 to 5.

I have mentioned theſe circumſtances in hopes of recommending the demonſtration of curves ſuited to the purpoſes of optics to the attention of mathematicians, which would be of great uſe to artiſts.

I ſhall conclude this paper with the deſcription of a new micrometer ſuited to the principle of refraction; being ſenſible that both principles have their peculiar advantages. Though the former part of this paper proves my partiality to the principle of reflection applied to micrometers, yet the very favourable opinion I have of the refracting teleſcope made me attentively conſider ſome means of applying a micrometer to it, which might obviate the errors complained of in the former part of this paper.

The application of any lens or medium between the object glaſs and its focus muſt inevitably deſtroy the diſtinctneſs of the image; I therefore have employed for the micrometer glaſs one of the eye glaſſes requiſite in the common conſtruction of the teleſcope; but if it ſhould be found neceſſary to apply an additional eye glaſs for the conveniency of enlarging the ſcale, I am able thereby to cor-

rect both the colours and ſpherical aberration of the firſt eye glaſs.

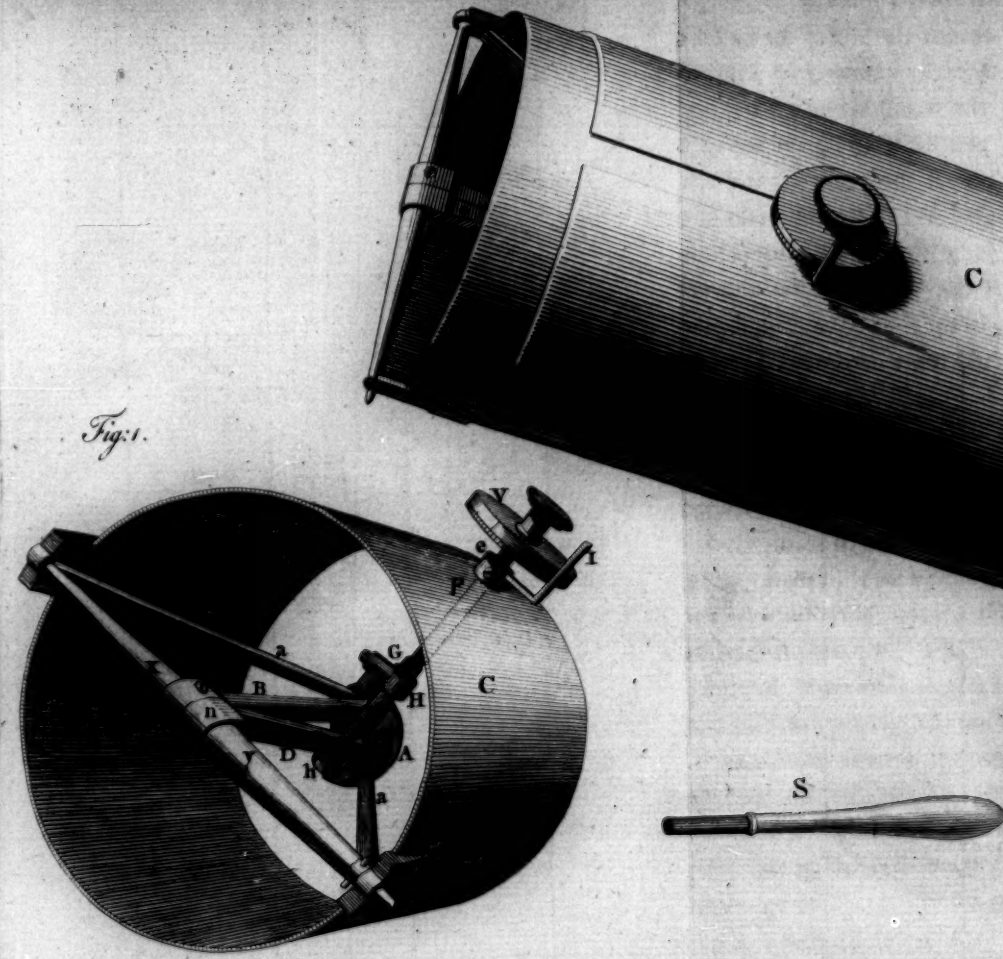
This micrometer is applied to the erect eye tube of a refracting teleſcope, and is placed in the conjugate focus of the firſt eye glaſs: hence ariſes its great ſuperiority to the object glaſs micrometer. It has been before obſerved, that if a micrometer is applied at the object glaſs, the imperfections of its glaſs are magnified by the whole power of the teleſcope; but in *this* poſition, the image being conſiderably magnified before it comes to the micrometer, any imperfection in its glaſs will be magnified only by the remaining eye glaſſes, which in any teleſcope ſeldom exceeds five or ſix times.

By this poſition the ſize of the micrometer glaſs will not be the $\frac{1}{100}$th part of the area which would be required if it was placed at the object glaſs; and, notwithſtanding this great diſproportion of ſize, which is of great moment to the practical optician, the ſame extent of ſcale is preſerved, and the images are uniformly bright in every part of the field of the teleſcope.

Fig. 4th, repreſents the glaſſes of a refracting teleſcope; *xy* the principal pencil of rays from the object glaſs o; *tt* and *ww* the axis of two oblique pencils; *a* the firſt eye glaſs; *m* its conjugate focus, or the place of the micrometer; *b* the ſecond eye glaſs, *c* the third, and *d* the

fourth, or that which is neareſt the eye. Let *p* be the diameter of the object glaſs, *e* the diameter of a pencil at *m*, and *f* the diameter of the pencil at the eye; it is evident, that the axis of the pencils from every part of the image will croſs each other at the point *m*, and *e*, the width of the micrometer glaſs, is to the diameter of the object glaſs as *ma* is to *go*, which is the proportion of the magnifying power at the [illegible] and the error cauſed by an imperfection in the micrometer glaſs placed at *m* will be to the error, had the micrometer been at *o*, as *m* is to *p*.

Fig. 3d, repreſents the mic[illegible]n-cave lens divided into two [illegible]oſs its center; one of theſe [illegible] frame B, and the other in the fra[illegible] two frames ſlide on a plate H, and are preſſed againſt it by thin plates *aa*; the frames B and [illegible] are moved in contrary directions by turning the button D; L is a ſcale of equal parts on the frame B; it is numbered from each end towards the middle with 10, 20, &c. There are two verniers on the frame I, one at M, and the other at N, for the conveniency of meaſuring [illegible] of a planet, &c. on both ſides the zero. [illegible] on both theſe verniers coincides at the ſame time with the two zero's on the ſcale L, and,


C
Fig: 1.
V
e
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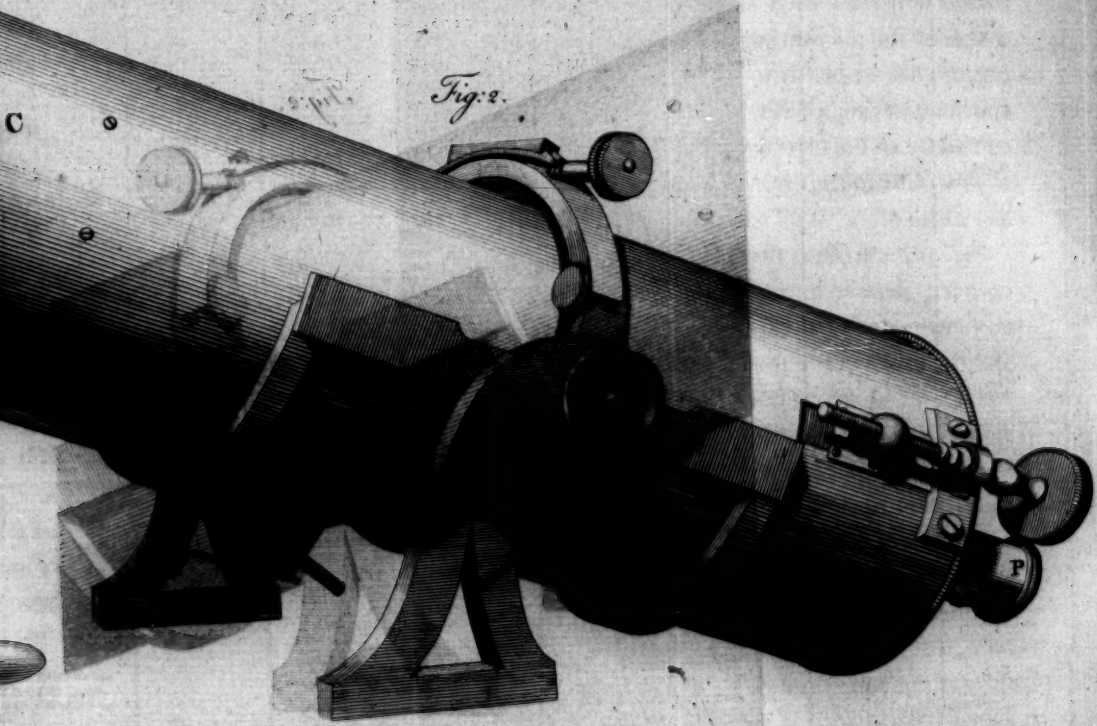


Fig: 2.

Fig: 3.d

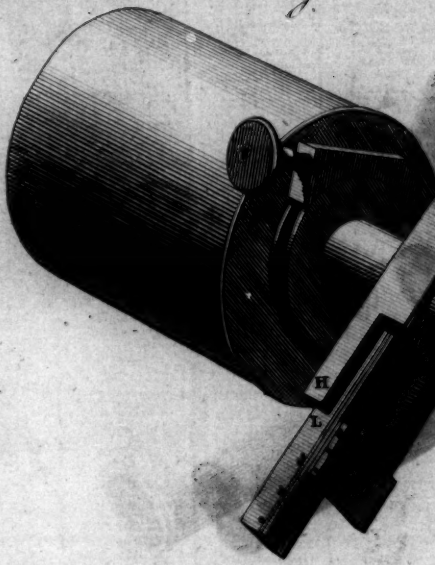
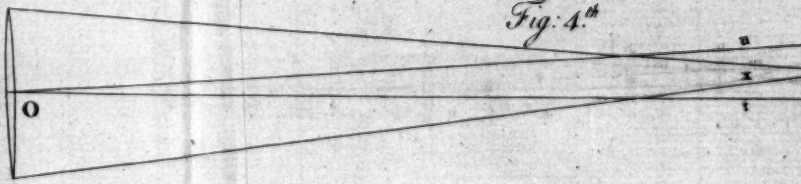
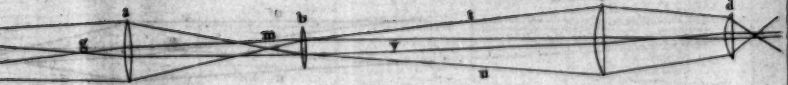


Fig: 4.th




a
m
b
c
d
e
f
g
n

and, if the frame is moved towards the right, the relative motion of the two frames is ſhewn on the ſcale L by the vernier M; but if the frame B be moved towards the left, the relative motion is ſhewn by the vernier N.

This micrometer has a motion round the axis of viſion, for the conveniency of meaſuring the diameter of a planet, &c. in any direction, by turning an endleſs ſcrew F, and the inclination of the diameter meaſured with the horizon is ſhewn on the circle *g* by a vernier on the plate V. The teleſcope may be adjuſted to diſtinct viſion by means of an adjuſting ſcrew, which moves the whole eye tube with the micrometer nearer or farther from the object glaſs, as teleſcopes are generally made; or the ſame effect may be produced in a better manner, without moving the micrometer, by ſliding the part of the eye tube *m* on the part *n*, by help of a ſcrew or pinion. The micrometer is made to take off occaſionally from the eye tube, that the teleſcope may be uſed without it.

XXVIII. *Account of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. In a Letter from the Abbé* Fontana, *Director of the Cabinet of Natural History belonging to his Royal Highneſs the Grand Duke of* Tuſcany, *to* Joſeph Prieſtley, *LL.D. F. R. S.*

Read April 16, 1779.

I TAKE the liberty of ſending you an account of ſome experiments which I made at Paris in the years 1777 and 1778 on the air extracted from various kinds of waters: ſome of the principal of which I thought proper to tranſcribe, that you might make uſe of them if you think that they are at all uſeful, and likewiſe lay them before the Royal Society.

I have extracted the air from the water of a well by means of common fire. The water was then made to boil in a large mattraſs of tin, which had a long tube of the ſame metal, which being bent into two different directions was with its extremity immerged in a tub of cold water. The mattraſs and its tube were intirely filled

filled with water: the air which came out of it was received into three different veſſels. The air of the firſt veſſel, by being ſhaken in water, was diminiſhed a little; the air of the ſecond was diminiſhed of half its bulk, or rather more; and the air of the third veſſel was diminiſhed exceedingly. The *reſiduums* of air that remained unabſorbed were more or leſs phlogiſticated.

Another time I obtained almoſt entirely fixed air, excepting a little which remained unabſorbed by water, and was partly phlogiſticated.

A third time the air of the water of a well, obtained as above, was made to paſs through mercury into a tube anointed with oil of tartar, and it occaſioned a cryſtallization juſt like that which the pureſt fixed air is uſed to do.

A fourth time I impregnated with this air a quantity of common water, which abſorbed its own bulk of it, and became by theſe means acidulous, exactly like water with the pureſt fixed air. This water turned the tincture of turnſole red, and precipitated the lime in lime water. Another time a light was ſucceſſively extinguiſhed, and a bird died inſtantly in this air.

The water of the river Seine, filtrated through ſand, as it is drunk at Paris, was treated in the ſame manner as the water of the well. The air extracted from that water was half abſorbed by water, when ſhaken in it; the remainder,

remainder, when treated with the taſte of nitrous air, gave II+4, H+1 (a); when the common atmoſpheric air treated with the ſame nitrous air gave II−4, II+8. It was therefore ſenſibly better than the atmoſpheric air, which, during three years of experiments made at Paris, I have conſtantly found to be inferior to the air of the Seine water, extracted as above.

Having repeated the experiment, I received the air into two different receivers. The firſt of which, by being ſhaken in water, was diminiſhed in the proportion of ten to ſeven; and by the teſt of nitrous air gave II−14, II+1, III+1, when the common air gave II−12, II+6, III+6.

The ſecond quantity of air was diminiſhed in the proportion of three to one; and when examined by the teſt of nitrous air gave II±0, III±0. From whence it may be concluded, that the firſt air was better than the atmoſpheric air; whereas the ſecond was worſe, and mixed with much fixed air.

Being in doubt whether the tin veſſel employed in the experiment above mentioned might not alter the nature of the air, &c. I made uſe of glaſs veſſels. Having therefore filled one of theſe veſſels, having a long neck bent in two directions, with the Seine water, I

(a) See p. 343. for an explanation of this meaſure.

obtained ſome air which ſeemed not ſenſibly diminiſhed when ſhaken in water. Having introduced one meaſure and 37 parts of this air into the tube uſed to try the diminutions, it gave with the nitrous air I+19, I+48, when the ſame quantity of nitrous and atmoſpheric air gave II+26, II+6: it is therefore certain, that the air extracted from Seine water is purer than common air.

Another time I extracted, in the ſame manner, and from the ſame water, the air; one meaſure and 24 parts of which being introduced into the tube, &c. and ſhaken, was reduced to one meaſure − 31 parts, that is, one fifth of it was abſorbed. Treated with the nitrous air it gave I−4, when equal meaſures of common and nitrous air gave I+0: it was therefore better than common air.

A third time I extracted the air, in the manner above mentioned, from the water of the river Seine, contained in three mattraſſes; this air was about one twenty-eighth of the bulk of the water, and it gave with the teſt of nitrous air II−14, II−9, III−9; when the common air mixed with nitrous air, as uſual, gave II−14, II+8, III+8. It is therefore clear, that the air extracted from the Seine water, by the action of fire in glaſs veſſels, is much better than common air, or than the air which is extracted from the ſame water when boiled in tin veſſels.

Another

Another time I filled a glass retort, which had a long and doubly bent neck, with Seine water. The water weighed about three pounds. The air that came out of it lost a quarter of its bulk by being shaken in water; and afterwards being tried with nitrous air it gave II−16, II−16, III−16, when common and nitrous air gave II−12, II+12.

This experiment being repeated, the air was diminished of one quarter by being shaken in water. One measure −16 parts of this air introduced into the measuring tube gave II−32, II−2, when common air and the nitrous gave II−28, II+4.

The water d'Arqeuil at Paris is considered as very pure. I filled the tin vessel above mentioned with it, and received the air that came out of it into three vessels. Being shaken in water, the first of them was diminished one fifth; the second, three fourths; and the third $\frac{15}{16}$ by the operation in water. A light burned with a flame, more luminous than in common air, in the first air after it had been shaken in water. This air being tried with the nitrous air gave II−10, II−10, III−10. The second gave II−10, II−17, III−30, when common and nitrous air gave II−2, II+14, III+14. The third air, before it was shaken in water, crystallized with the oil of tartar like fixed air. An equal bulk of it was absorbed by

water, which by this mixture became acidulous; it precipitated the lime in lime water, extinguiſhed a light ſeveral times, and killed an animal inſtantly. It is therefore partly fixed air, and partly air which is not only better than common air, but likewiſe than that extracted from Seine water, even when this laſt has been boiled in glaſs veſſels.

The experiments being repeated with the ſame water of Arqueil, but in glaſs veſſels, the air obtained, after being ſhaken in water, was much better than that obtained from the ſame matter, when boiled in veſſels of tin.

I have alſo extracted the air from diſtilled water in glaſs veſſels, and having ſhaken one meaſure – 32 parts of it in water, it was reduced to one meaſure – 35 parts. With the teſt of nitrous air it gave I–6, when equal parts of common and nitrous air gave I–2, which ſhewed that it was better than common air.

I extracted the air again in the manner above deſcribed; but it was not ſenſibly diminiſhed when ſhaken in water. Two meaſures – 49 parts of it, with the teſt of nitrous air, gave I–2, I+8, when common air, &c. gave I+1, I+18: It is therefore better than common air.

I extracted the air from a great quantity of diſtilled water in the uſual manner, and found that it did not ſen-

ſibly diminiſh in water. With nitrous air it gave II−14, II−25, II+25, when common and the ſame nitrous air gave II−14, II+10, III+10; conſequently it was dephlogiſticated air, *viz.* purer than the air of the Seine and Arqueil, which are much better than common air.

I had the curioſity to try, whether any difference would ariſe from boiling diſtilled water in a matraſs of tin inſtead of glaſs veſſels; and found, that the firſt air was diminiſhed one tenth by being ſhaken in water, and afterwards with the nitrous air gave II−13, II−16, III−18, when common air gave II−12, II+8; which ſhews that it was dephlogiſticated air, but not ſo good as that extracted from the ſame water when boiled in glaſs veſſels. The ſecond quantity of air was not ſenſibly diminiſhed in water, and with nitrous air gave II−13, II−20, III−30; that is, it was more dephlogiſticated than the firſt.

The air extracted from diſtilled water is to that extracted from the water of the river Seine as 13 to 32 nearly; whence diſtilled water does not give more air than one ſixtieth of its bulk: but as the air extracted from the water of the Seine is half fixed air, it may be concluded, that the quantity of reſpirable air produced by both kinds of waters is nearly the ſame, and that they only differ a little in purity. It is however true, that other ex-

periments have ſhewn me that water in general abſorbs about twice as much of dephlogiſticated as of common air; for which reaſon, I think, that the reſpirable air of Seine water is rather leſs than that of diſtilled water. Accordingly I have found, that Seine water, after it has been boiled for a long time, abſorbs in forty days about one fourteenth of its own bulk of dephlogiſticated air, when in the ſame length of time it does not abſorb more than one twenty-eighth of common air. This ſeems to be an experiment of very great conſequence, and is much worth notice; eſpecially as it diſcovers a new characteriſtic by which dephlogiſticated air may be diſtinguiſhed from common air; and ſhews, that water abſorbs a greater quantity of thoſe kinds of air, which contain a leſs quantity of phlogiſton.

It muſt however be obſerved, that it is impoſſible to determine exactly the quantity of air that is extracted from veſſels filled with water, by means of fire; becauſe a portion of the air is abſorbed by the water of the tub in the act of its coming forth. It will certainly be more exact to receive the air in veſſels immerſed in quickſilver; but then there are many other inconveniencies to encounter.

It may be almoſt ſuperfluous to mention, that the above related experiments are very uſeful in explaining

the reaſon why ſome kinds of water have a peculiar ſharp taſte more than others; and eſpecially why ſome of them precipitate the lime in lime water, rendering it a calcareous earth, and change the tincture of turnſole into a red colour, as I have generally experienced with the well waters at Paris. We may alſo explain from hence, why ſome kind of waters can diſſolve iron, and keep it in diſſolution without depoſition; whereas other kinds of water are incapable of doing it, at leaſt do it much leſs than the pureſt diſtilled water. This is ſoon diſcovered by boiling the water, which will then depoſit the iron which before was diſſolved.

It will be ſufficient, for the preſent, to mention, that I have not only extracted from waters the different kinds of air they contained naturally, but have likewiſe made various experiments upon waters deprived of air, which, being expoſed, have again imbibed the atmoſpherical air, as I hinted above. I have determined the quantity and quality of thoſe airs. In general, I may ſay, that diſtilled water, deprived of air, imbibes again an equal quantity of air of the ſame kind as that it had loſt, and that in leſs than fifty days. Other kinds of water do the ſame, but with this difference, *viz.* that the air they abſorb, after being boiled, is better than that they have loſt; and in this particular they come very near to the nature of diſtilled water itſelf.

If the waters deprived of air are expoſed to common air held in receivers in contact with quickſilver, the air which remains unabſorbed is ſo much more phlogiſticated as a leſs quantity of it remains in the receiver. This experiment deſerves conſideration.

By means of pure water, eſpecially diſtilled water, common air may be changed into dephlogiſticated air, that is, into air much more ſalubrious than the beſt common air which we breathe; and this, for what I know, is the only means of meliorating common air: for all the artificial methods (great numbers of which I have tried) have proved either uſeleſs or noxious, but never ſuch as promiſed to be of any great utility to human kind.

Though I have long thought of applying thoſe experiments to ſome uſe for the purpoſes of life, the want of time and a proper apparatus has hitherto hindered my doing any thing; I now begin to be in hopes I ſhall be able to do ſomething. In the mean time I think it of ſome importance to have it known, that water not only poſſeſſes the property of diminiſhing the noxious part of tainted air, but has alſo the power, and that in a very high degree, of dephlogiſticating common air; which muſt certainly be one of the methods by which nature keeps the atmoſphere in a ſtate conſtantly fit to ſupport animal life, it being certain, that the water in various circumſtances

circumſtances muſt loſe either a part or the whole of that air which it hath abſorbed from the atmoſphere.

It may with ſome reaſon be ſuſpected, that in my experiments of extracting the air from water by the action of fire, the air might be conſiderably altered by the vapour of the water itſelf. As this difficulty was of ſome force, I endeavoured to remove it in the following manner. I introduced into a tube, through water, a quantity of common air of known goodneſs, and I cauſed the ſteam of water boiling in a matraſs, from which the air had been previouſly extracted, to paſs through it. The heat of the ſteam ſometimes made the water occupy above five times the ſpace it did when cold; yet the air ſo treated was not at all altered by it, as appeared by the teſt of nitrous air. The event of the experiment, although repeated various times, was conſtantly the ſame.

I muſt obſerve, laſtly, that having once cauſed the air of boiling water to paſs into receivers filled with, and ſtanding in, quickſilver, I found that the air was better than uſual. I have obſerved the ſame thing when I have cauſed the air to go through diſtilled water into receivers filled with it: which obſervation, if the event of the experiment is conſtantly the ſame, induces me to believe, that the air loſes ſome of its good properties by going through water not very pure; or, which ſeems to be rather

rather more probable, that a quantity of air leſs good is, by the action of the vapours and the heat, extricated from that impure water, and is mixed with the air that comes out of the matraſs; from whence this air is debaſed.

I muſt not omit to mention a new character of equal importance with that which diſtinguiſhes the dephlogiſticated from the common air. This new character has been equally unknown, and deſerves the attention of philoſophers, becauſe at the ſame time it diſcovers a new property of the atmoſpherical air, which I ſhould never have ſuſpected if experience had not offered it to me.

I have found, that common air ſhaken in water, inſtead of being diminiſhed is ſenſibly increaſed in its bulk. The increaſed ſpace is in proportion to the time the air is ſhaken in water, and it begins to be ſenſible even from the beginning, that is, after a few ſeconds. This augmentation I have ſometimes brought to be one twelfth of the bulk of the air, and even more: it muſt, however, be confeſſed, that I met with great variety in the experiments of this kind made at different times. After that the bulk of the air ſhaken in water is increaſed to a certain degree, it then begins to decreaſe continually; and, in proportion to this decreaſe, the air becomes gradually leſs good. When the experiment is tried in cloſe veſſels, the diminution

nution cannot be obſerved; but I ſhall reſerve, for another opportunity, to ſpeak of the laws and cauſes of thoſe diminutions and augmentations, and the differences obſerved between common and other kinds of air, and when the experiments are tried in water, &c.

For the preſent I ſhall only mention, that if the dephlogiſticated air be ſhaken in the tube, in the manner above mentioned, not only it does not increaſe its bulk, but it begins to diminiſh from the very beginning of the operation, and it continually loſes more and more of its bulk, and with its bulk of its purity.

This laſt mentioned property of the dephlogiſticated air ſeems to ſhew, that this is a fluid much different from common air, becauſe it has its peculiar properties by which it differs from common air not from more to leſs only, but entirely; as is ſhewn by the property this fluid has of being abſorbed by water; whereas common air receives an increaſe of bulk and elaſticity by being ſhaken in water.

All that I have been ſaying above, in order to give an idea of my method, and the words I uſe to expreſs the diminutions made by the mixture of nitrous air and other kinds of reſpirable air, is not ſufficient to obtain reſults conſtant and certain, ſo as to deduce any conſequences from them. Even after that all the elements are

corrected,

corrected, and all the cauſes of error hitherto unknown or neglected by the moſt diligent obſervers, are avoided (which cauſes ſenſibly alter this kind of experiments) it is abſolutely neceſſary to follow always a conſtant and equal method, not only in the act of introducing the various kinds of air into the tube, but alſo after the mixing of the two kinds of air. The leaſt variation of circumſtances cauſes very great variations in the reſults of the experiments, and theſe variations of circumſtances are ſo minute that I never ſaw any of the perſons that obſerved my experiments who could diſcover them, although apprized of my deſign. The neglect alone of this uniformity of operation may occaſion an error of from 20 to 50 parts and upwards in the experiment with common air; but with dephlogiſticated air the error is incomparably greater, ſo much that the ſame quality of air a moment after may decompoſe even a double quantity of nitrous air; ſo that the pureſt common air would appear to be noxious, and phlogiſticated air; and the dephlogiſticated air would appear leſs good, and even noxious: for, by the teſt of nitrous air, it might appear little different from a mixture of dephlogiſticated and phlogiſticated air.

I ſhall take another opportunity to ſpeak of all the particulars relative to my method; but for the preſent I

must mention, that, after using all the cautions I am master of; the greatest error in the diminutions is not greater than one sixtieth of the common air introduced in the tube; so that, after having made five or six experiments successively, the probable error is so small that it may be safely neglected. But, if one chose to operate upon a quantity of air nine or ten times greater than that I commonly use, the error could not be 1000th part of the quantity of the air, which quantity would not be more than a few cubic inches. In the description of my method I shall also mention the means by which I obtain nitrous air of a nearly equal and constant goodness, and in what manner I can refer my experiments to some constant standard. For want of this method it is that we are not certain of the observations made about the salubrity of common air in different places, and that no tables of its changes have been made.

I have not the least hesitation in asserting, that the experiments made to ascertain the salubrity of the atmospherical air in various places, in different countries and situations, mentioned by several authors, are not to be depended upon; because the method they used was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the results very different from one another.

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When all the errors are corrected it will be found, that the difference between the air of one country and that of another, at different times, is much leſs than what is commonly believed, and that the great differences found by various obſervers are owing to the fallacious effects of uncertain methods. This I advance from experience; for when I was in the ſame error, I found very great differences between the reſults of the experiments of this nature which ought to have been ſimilar; which diverſities I attributed to myſelf rather than to the method I then uſed. At Paris I examined the air of different places at the ſame time, and eſpecially of thoſe ſituations where it was moſt probable to meet with infected air, becauſe thoſe places abounded with putrid ſubſtances and impure exhalations; but the differences I obſerved were very ſmall, and much leſs than what could have been ſuſpected, for they hardly arrived to one fiftieth of the air in the tube. Having taken the air of the hill called Mont Valerien at the height of about 500 feet above the level of Paris, and compared it with the air of Paris taken at the ſame time and treated alike; I found the former to be hardly one thirtieth better than the latter. In London I have obſerved almoſt the ſame. The air of Iſlington and that of London ſuffered an equal diminution by the mixture of nitrous air; yet the

air of Iſlington is eſteemed to be much better. I have examined the air of London, taken at different heights; (for inſtance, in the ſtreet, at the ſecond floor, and at the top of the adjoining houſes) and have found it to be of the ſame quality. Having taken the air at the iron gallery of St. Paul's cupola at the height of 313 feet above the ground, and likewiſe the air of the ſtone gallery which is 202 feet below the other; and having compared theſe two quantities of air with that of the ſtreet adjoining; I found, that there was ſcarce any ſenſible difference between them, although taken at ſuch different heights.

In this experiment a circumſtance is to be conſidered, which muſt have contributed to render the above mentioned differences more ſenſible; this is, the agitation of the air of the cupola, for there was felt a pretty briſk wind upon it, which I obſerved to be ſtronger and ſtronger the higher I aſcended; whereas in the ſtreet, and indeed in all the ſtreets I paſſed through, there was no ſenſible wind to be felt. This experiment was made at four in the afternoon, the weather being clear. The quickſilver in the barometer at that time was 28,6 inches high, and FAHRENHEIT's thermometer ſtood at 54°. After having related all theſe circumſtances, it will be neceſſary to give the mean reſult of all the various experiments

periments made upon each of those quantities of air treated after my method with the nitrous air. The air of the street gave II−13, II+6; the air of the stone gallery, which was 202 feet high, gave II−14, II+5; and the air of the iron gallery, which was 313 feet high, gave II−14, II+5. The results of the two last experiments are exactly the same; and that of the first is hardly at all different from them. Mr. CAVALLO, who has shewn the literary world his ability in examining nature, assisted me in those experiments, so that a mistake can hardly be suspected. From this we clearly see, how little the experiments hitherto published, about the differences of common air, are to be depended upon. In general I find, that the air changes from one time to another; so that the differences between them are far greater than those of the airs of different countries, or different heights; for instance, I have found that the air of London, in the months of September, October, and November, 1778, when treated with the nitrous air, gave II−6, II+15, which is a mean result of many experiments which differed very little from each other. The 26th day of November last, I found the air for the first time much better, for it gave II−12, II+12; but the 14th of February last, the air gave II−18,

11−18, 11+7; from whence it appears, that the air of this 14th of February was better than it had been for ſix months before. There can be no doubt of the accuracy of the experiments, becauſe I compared the air taken at different times with that which I had firſt uſed in the month of September, and which I had preſerved in dry glaſs bottles accurately ſtopped. Now if the formulæ expreſſed above are compared together, it will be found, that the difference between the firſt terms is of twelve parts, and that between the latter of ſeven; that is, of one tenth and one twenty-fourth of the whole quantity of air: which are much greater differences than thoſe mentioned above. Notwithſtanding this, I could not perceive any particular change of health, or facility of breathing, ariſing from thoſe changes of the ſalubrity of the atmoſpherical air; and I am informed, that no particular diſeaſes appeared which could indicate any remarkable change of air.

Nature is not ſo partial as we commonly believe. She has not only given us an air almoſt equally good every where and at every time, but has allowed us a certain latitude or a power of living and being in health in qualities of air which differ to a certain degree. By this I do not mean to deny the exiſtence of certain kinds of noxious

air in ſome particular places; but only ſay, that in general the air is good every where, and that the ſmall differences are not to be feared ſo much as ſome people would make us believe. Nor do I mean to ſpeak here of thoſe vapours and other bodies which are accidentally joined to the common air in particular places, but do not change its nature and intrinſical property. This ſtate of the air cannot be known by the teſt of nitrous air, and thoſe vapours are to be conſidered in the ſame manner as we ſhould conſider ſo many particles of arſenic ſwimming in the atmoſphere. In this caſe it is the arſenic, and not the degenerated air, that would kill the animals who ventured to breathe it.

In this place, therefore, I do not mean to ſpeak of thoſe changes which do not immediately alter the nature of the air itſelf. The other ſtates of that fluid are of another kind, and they are not to be examined by means of nitrous or inflammable air (the uſes of which laſt, I ſhall ſhew on another occaſion). The ſame thing may be ſaid of thoſe vapours or particles which may be good for reſpiration, and do not change the nature of the air. Some vegetables, for inſtance, can diffuſe through the air ſuch exhalations as may be of real uſe to the animal œconomy when they

are

are breathed for a long time, or imbibed by the pores of the ſkin. I remember to have often put various flowers, as roſes, pinks, &c. in veſſels full of common air confined by quickſilver, where I left them for ſeveral hours; after which time I found, that the air was not at all altered, but that various animals ſeemed to breathe it very well, notwithſtanding that the flowers filled the greateſt part of the veſſels. On the contrary, I have found, that the vapours ariſing from lime ſlacked in water, either do not alter the air at all, or very little; though when breathed with the air they occaſion the death of animals.

I would not have any body ſuppoſe, that I think it of little importance to know the goodneſs of the atmoſpherical air, and the changes it undergoes. On the contrary, I believe it to be a very uſeful inquiry for mankind, becauſe we do not yet know how far one kind of air more than another may contribute to a perfect ſtate of health; nor at what time ſmall differences may become very conſiderable, when one continues to breathe the ſame kind of air for whole years, eſpecially in ſome kind of diſeaſes. An exact method of examining the goodneſs of common air may even be uſeful to poſterity, in order to aſcertain whether our atmoſphere degenerates in a length of time. This curious inquiry, together

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with the method, &c. are the production of this eighteenth century; and our descendants must have some gratitude for the philosophers who found out, as well as for those who improved it. If our ancestors had known and transmitted it to us, we should, perhaps, at present be able to judge of one of the greatest changes of our globe, of a change which very nearly interests human life.

XXIX. *Account of ſome Experiments in Electricity. In a Leter from Mr.* William Swift *to the Rev. Dr.* King, *F. R. S.*

Read April 29, 1779.

SIR, Greenwich, April 28, 1779.

BEING encouraged by the attention the Royal Society did me laſt year to give to the account of ſome electrical experiments I had made, which you were ſo good as to preſent to them, I take the liberty to beg their indulgence again, and hope you will be pleaſed to lay before them ſome new improvements I have made in my electrical apparatus. I ſhall eſteem myſelf very happy if what I have done is worthy the farther attention of that moſt learned and reſpectable body.

One particular addition I have made to the apparatus conſiſts in what I call an anti-conductor: it is exactly like the prime conductor, but it is fixed to the cuſhion of the machine, and conſequently, when the cylinder is put in motion, the anti-conductor is charged negatively, that is, the electric matter is diminiſhed therein in the ſame proportion

portion as it is increaſed in the prime conductor in the ſame time.

Another thing peculiar to this machine is, that the whole is inſulated, ſo that being able to collect the electric matter without any connection with the earth, and having at the ſame time bodies or conductors poſitively and negatively electrified, I apprehend I am enabled by this apparatus to exhibit many experiments more analogous to the natural effects of lightning from the clouds, than it is poſſible to do with only one conductor poſitively electrified; becauſe in nature clouds are conſtantly flying in the air which are differently electrified, and, diſcharging themſelves in each other, produce the lightning often ſeen in the atmoſphere.

I have annexed a ſlight ſketch of the apparatus: *a* is the glaſs cylinder; *b* the prime conductor, on two glaſs ſtems; *c* the anti-conductor, on two glaſs ſtems, connected with the cuſhion of the machine; *dd* is a pole or round ſtaff, well covered with metal, with a ball at each end, which hangs over the two conductors *b* and *c*; this ſtaff is faſtened to the croſs pole *f*, which is ſuſpended by a ſilk line in ſuch a manner that the ſtaff *dd* is equally balanced.

It may be proper to mention a few common experiments and obſervations, to ſhew, that the two conductors

are charged and differently charged, that is, the one poſitively or *plus*, the other negatively or *minus*, as ſoon as the cylinder is put in motion by turning the wheel; though, perhaps, this will not be doubted by any perſons converſant in electrical experiments, unleſs they have been fully perſuaded, that the electric matter cannot be excited without connecting the machine with the earth, which is not the caſe in this apparatus, for the glaſs cylinder and both conductors are inſulated.

1. When the cylinder moves, and a body approaches the prime conductor, ſuch body will draw a ſpark from that conductor at the ſame diſtance, and conſequently of the ſame length as will be drawn from the approaching body by the anti-conductor. And a pith ball is equally attracted by both, which ſufficiently ſhews, that both conductors are charged or electrified.

2. The following common experiments will ſhew, that they are differently electrified.

I take a wire with a ſmall piece of cocoa wood, about one inch and a half long, pointed, faſtened to one end of the wire, and connecting the other end to the anti-conductor; as ſoon as a conducting body approaches it there is a bright ſpark reſembling a ſtar, which appears to ſettle upon the end or point of the wood; but when the wire is connected with the prime conductor, there iſſues

from the end of the wood a pencil of rays diverging to the point towards the approaching body, which, I apprehend, demonstrates the conductors to be differently electrified.

3. It may also be seen by another experiment. I take two jars, coated as in the Leyden experiment, and charge one by the prime conductor, the other by the anti-conductor; the first will be positively, and the second negatively electrified; which is proved by applying a discharging rod to the balls connected with the inside of the jars, when both immediately discharge themselves, which they would not do, if both jars were charged from the same conductor.

These experiments I only mention, to shew, that the two conductors are both electrified, and with this difference, that the one has more, the other less, electrical matter than in its natural state.

4. When the pole *dd* is let down by slackening the silk line on which it hangs within the sphere of action of the two electrified conductors, and being equally balanced remains *in equilibrio* over both, as soon as the cylinder begins to turn, the pole vibrates regularly towards each conductor, and as it approaches the one or the other gives a flash or spark.

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In this ſtate if a ball be preſented to either conductor, it makes no alteration; the pole continues to vibrate as before, even though a flaſh comes on the ball: but when a point is preſented to either of the conductors *b* or *c*, the vibrations of the pole begin to abate, and no flaſh comes to the point. And as ſoon as any connection is made with the earth, the point being preſented to one conductor, the pole attaches itſelf to the other with continual flaſhes.

5. I take two glaſs globes coated *ee*, and ſuſpend them towards the ends of the pole *dd*. On turning the cylinder, the pole will vibrate, and the globes will, in a ſhort time, become charged; each globe will have its outward coat charged in the ſame way as the conductor over which it hangs, as will appear by diſcharging them into each other with a common diſcharging rod. This experiment ſhews the poſſibility of compreſſing the electric matter, though the globes are perfectly inſulated.

6. While the machine remains in this ſtate, let the prime conductor *b* be connected with other clouds, fig. 2. which are made to move over houſes having conductors terminated with points; the globe *e* over the prime conductor will be thrown upwards or repelled, and the other globe only will receive a charge from the anti-conductor *c*: or if, on the contrary, the anti-conductor *c* be

connected with the flying clouds, the oppoſite globe only will receive its charge from the prime conductor. But if balls inſtead of points be made to terminate the conductors of the houſes, the globes *e e* will vibrate till both be equally charged.

7. Or if we take off the globes and place a large jar or battery to each conductor, and charge them by turning the cylinder till the electrometer riſes to ninety degrees, the ſtrokes of the vibrations of the pole *d d* become very ſtrong; then, by means of an inſulated rod, I place a point on the upper part of either conductor where the knob of the pole ſtrikes, and no exploſion will come on that point. But if, inſtead of a point, a ball be ſo interpoſed, there will be an exploſion; and the larger the ball is, the greater the exploſion will be. In the latter part of this experiment, when the ball is interpoſed, if any perſon applies his finger to any wire on the board *w w w*, or to the ſolid pieces of marble ſtanding on the board, he will be ſenſibly affected with a ſhock every time the exploſion happens; but when the knob is in contact with the point in the former part of the experiment, as there is no exploſion, neither will his finger be affected on the marble or any part of the wire.

8. Another experiment, which ſeems to agree in effect with all the foregoing, is as follows.

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From either the prime conductor, or from the anti-conductor, I ſuſpend a metal ball which can ſwing like the pendulum of a clock; I then place on a level with it, at the diſtance of four or five inches from it, another ball fixed and connected with the earth; as ſoon as the cylinder begins to turn, the ſwinging ball begins to be attracted, and ſtrikes with conſiderable force againſt the fixed one, emitting a ſpark or flaſh at the ſame time.

In ſuch poſition if a point be put the vibrations of the ſwinging ball will immediately begin to ceaſſen, and it ſoon becomes intirely at reſt, no flaſh or ſpark happening from the inſtant the point is put there.

9. On one of the knobs or balls at the end of the pole *dd*, I put a point turned downwards towards the conductor, and as ſoon as the cylinder moves it is thrown upwards or repelled, and the oppoſite knob or ball adheres to the conductor under it; but when points are put on both knobs and turned downwards or towards the conductors, the pole *dd* will remain unmoved, notwithſtanding all the poſſible friction which can be given by turning the cylinder.

10. To render theſe experiments more analogous to the natural phenomena of lightning and rain deſcending from the clouds, I place a veſſel of water inſulated, and as the clouds (fig. 2.) being charged paſs along the frame,

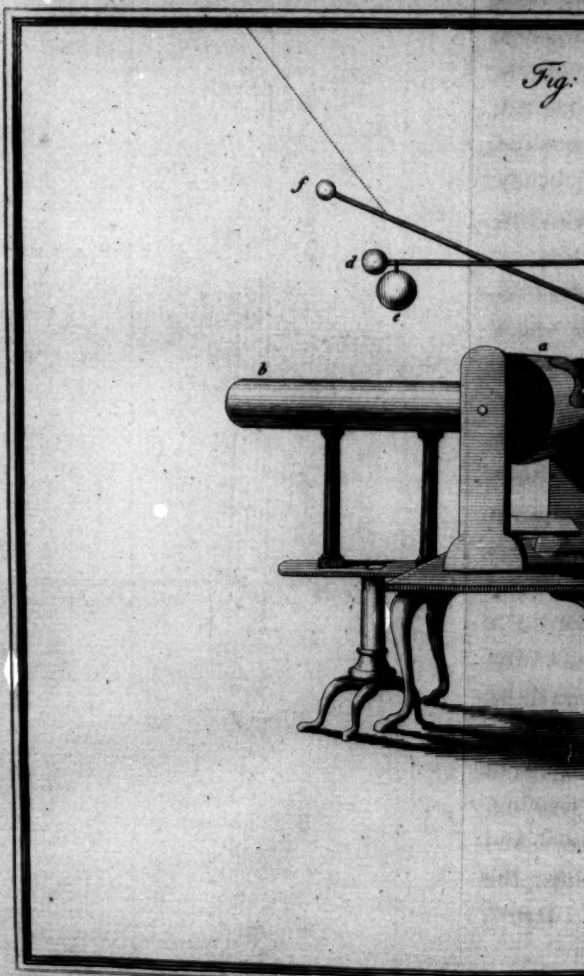
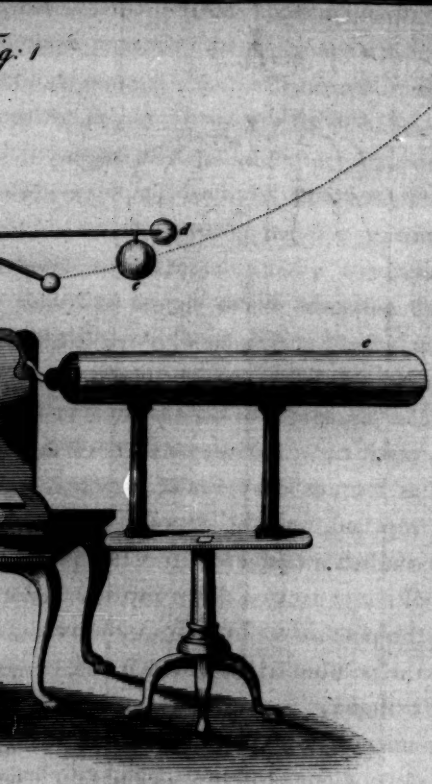


Fig.
f
d
e
b
a

Fig: 1



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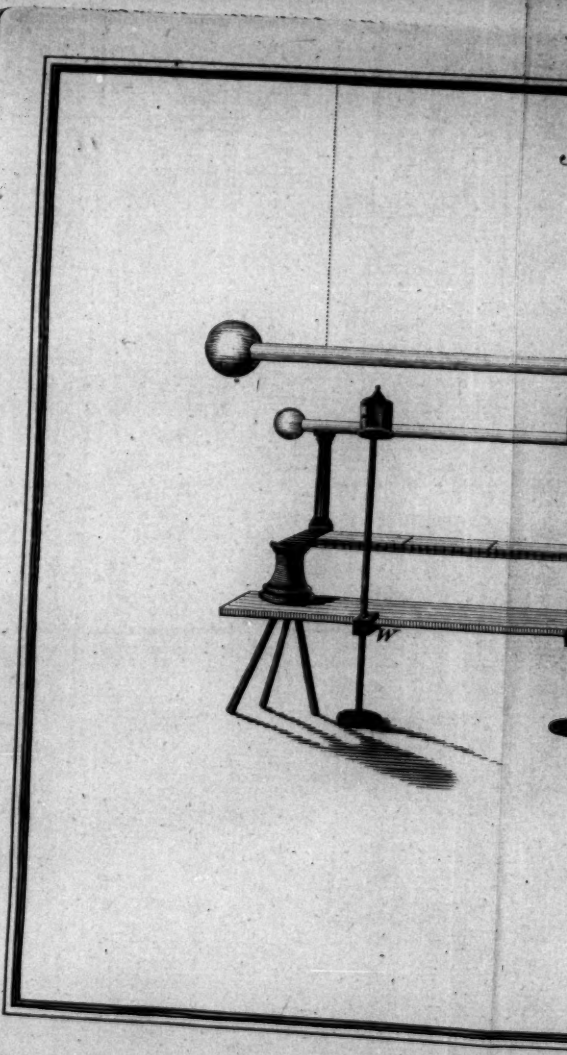
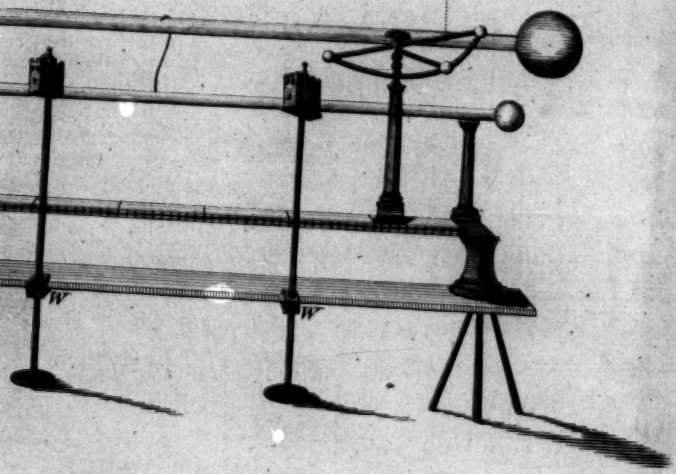


Fig. 2



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frame, they revolve over another cloud; which is introduced in the place of the middle houſe: all the houſes being taken away through this cloud, the water in the inſulated veſſel is made to paſs and to deſcend in a ſtream. When a ball is interpoſed between this cloud and the revolving ones, there will be frequent flaſhes upon it; but when, inſtead of a ball, a point is put there, the electric matter paſſes off gradually and ſilently without any flaſh.

Thus, SIR, I humbly apprehend, the whole current of theſe experiments tends to ſhew the preference of points to balls, in order to diminiſh and draw off the electric matter when excited, or to prevent it from accumulating; and conſequently the propriety or even neceſſity of terminating all conductors with points, to make them uſeful to prevent damage to buildings from lightning. Nay the very conſtruction of all electrical machines, in which it is neceſſary to round all the parts, and to avoid making edges and points which would hinder the matter from being excited, will, I imagine, on reflection, be another corroborating proof of the reſult of the experiment themſelves.

I am, &c.

XXX. Sitodium *inciſum et macrocarpon, uſuſque fructuum qui exinde naſcuntur, deſcripta a* Carolo Petro Thunberg, *M. D.; communicated by* Joſeph Banks, *Eſq, P. R. S.*

Read May 13, 1779.

AUCTORES, qui deſcriptionem dederunt hujus arboris vel fructus ejus, quantum ſcio, pauci ſunt.

RHEEDE, HENR. in Horto Malabarico, Amſt. annis 1678 et ſeq, fol: *macrocarpon* deſcribit.

RUMPHIUS, GEORG. EVERH. M. D. et medicus Amboinenſis annos ab hinc octoginta, utriuſque ſpeciei hiſtoriam dedit, quæ invenitur in Herbario Amboinenſi, a Prof. LAUR. BURMANNO, annis 1741 ſeq. publici juris facto.

ZANONIUS, JACOB. in Hiſtoria ſtirpium rariorum, Bonon. 1742, fol. *macrocarpon* memorat.

HAWKESWORTH, JOHN, Voyages round the World, Lond. 1773, vol. III. 4°, *inciſi* mentionem facit.

ELLIS, JOHN, fructum hujus deſcripſit anno 1775; in a Deſcription of the Mangoſtan and the Bread-fruit, Lond. 4°.

FORSTER,

FORSTER, JOH. REINH. et GEORG. in Characteribus generum Plantarum, p. 101 et 102. tab. 51. Lond. 4°. 1776, descriptionem genericam et figuram dederunt.

Ego, anno 1773, Batavine Indorum commoratus, arbores hasce characteribus botanicis descripsi et ad genus secundum sexuale systema reduxi, quae descriptio sub nomine Radermachiae incisae et integrae in actis Stockholmiensibus, anno 1776, p. 250. invenienda est. Eodem anno, 1775 scilicet, semina *S. macrocarpi* misi ad hortum medicum Amstelodamensem. Postea redux e Japonia, anno 1777, plantas minutas vivas utriusque speciei plantatas ad eundem hortum e Batavia transmisi. Deinde in Europam anno 1778 ipse revertens insignem numerum mecum duxi e Ceilona, tam plantarum utriusque speciei vivarum, quam alterius speciei seminum, quae pluribus modis conservare studui, scilicet:

1°, In chartulis, quae theca includebantur, et aliquando aeri in umbra exponebantur.

2°, In lagenis vitreis bene obturatis.

3°, In cera.

4°, In arena sicca. Ultimae hae duae methodi optimae fuerunt. Praeterea

5°, Sub ipso itinere singulis mensibus semina terrae mandavi, ut sensim excrescere possent sub vario coelo et vario anni tempore.

SYNONYMA varia, quibus inſignitur hoc genus a variis nationibus et variis in locis, ſunt ſequentia:

Cingallis, Herreliga.

Mallabaris, Plápallam, Tsjakamaram, Pilau.

Malais, Succong, Tjampeda, Tsjampedaha, Nanca.

Portugallis, Jaka, Jaccas, Jaqua, Jaqueira.

Hollandis, Syrſack, Schooſack, Schrootſack, Broodboom.

Anglis, Bread-fruit Tree.

Botanicis, Soccus, Saccus, Radermachia, Artocarpus, Sitodium.

Deſcriptio generica.

FLORES MASCULI.

Cal. nullus. *Spadix* cylindricus vel ſubclavatus, ſenſim incraſſatus, floribus undique tectus.

Cor. *Petala* duo, oblonga, concava, obtuſa, villoſa, filamentum includentia.

Stam. *Filamentum* unicum intra ſingulam corollam, filiforme, diaphanum, corollæ longitudine.

Antherа pyramidalis.

FLORES FEMINEI.

Cal. nullus. *Pericarpium* ovatum, germinibus tectum.

Cor. nulla.

Piſt.

Pist. *Germina* convexa, frequentissima, sexangulata.
Stylus filiformis, persistens.
Stigma unum vel duo, capillaria, revoluta, lineam longa.

Peric. *Bacca* ovata, muricata, carnosa, multilocularis.
Semina multiplici serie imbricata, ovata, oblique triquetra, carne molli involuta.

Character genericus. ♂ Cal. o. Corol. dipetala.
♀ Cal. o. Corol. o. Styl. 1.
Bacca multilocularis.

Habitatio insularis ut in Ambona, Banda, et aliis Moluccis, Sumatra, Java, Ceilona, Maldivicis aliisque.

Descriptio specifica.

1. *S. incisum*, foliis incisis; ramulis floriferis.

Soccus lanosus. Soccus capas. RUMPH. Herbar. Amboin. P. I. pag. 110. tab. 32.

Soccus granosus. Soccun bidji. RUMPH. Herb. Amb. P. I. pag. 112. tab. 33.

Soccus silvestris. Soccun utan. RUMPH. Herb. Amb. P. I. pag. 114. tab. 34.

Bread-fruit. HAWKESWORTH's Voyages, vol. II. p. 80. tab. N° 11.

Radermachia

Radermachia inciſa. Ego Act. Stockholmenſ. anno 1776, pag. 253.

Malais. Tjampeda, Nanca bibulang, Succong, Succon radja, Succon timbul.

Hollandis, Bataviæ. Schoorſack, Syrſack, Broodboom. Ceilonæ. Syrſack, Maldiviſche Syrſack.

Creſcit ſpecies hæc ſponte in inſulis Ambon. Banda, et aliis Moluccis, in Java in et circum urbem Bataviam ſatis vulgaris, in inſulis Maldivicis alibique; culta in Mallabaria ut Surattis, ad promontorium Comorin; in Coromandelia ut Sutucorin, Tranquebariæ, Nagapatnam; inque Ceilona circum Colombo, Gale, Maturam, Jafnam, Trinquilimale, in inſulis Marianis, &c.

Floret fructuſque maturos profert, bis in annum, primis octo menſibus.

Caulis arboreus, erectus, craſſitie hominis, altitudine quinqueorgyali. *Rami* faſtigiatim oppoſiti, ſubverticillato-quaterni, patentes. *Ramuli* ſubverticillato-quaterni, floriferi.

Folia alterna, petiolata, oblonga, ſupra medium profunde inciſa, 9-lobata, integerrima, villoſo-ſcabrida, patentia; ſupra viridia nervis pallidis; ſubtus pallida; bipedalia, pedem lata; juniora plicata, minora, viſcida. *Petiolus* craſſus, ſubtriqueter, villoſus, pollicaris. *Stipulæ* juniora folia involventes, duæ, ſeſſiles, lanceolatæ,

acuminatæ, concavæ, integerrimæ, intus glabræ, extus hirsutæ, deciduæ, palmares.

Flores in ultimis ramulis, masculi et feminei distincti in eodem ramulo, pedunculati.

Pedunculus subteres, villosus, erectus, bipollicaris, crassitie digiti.

Spadix spithamæus, cernuus, deciduus.

Pericarpium magnitudine capitis infantis, sterile vel seminibus sterilibus farctum.

γ. S. macrocarpon, fol. indivisis trifidisve; caudice ramisque floriferis.

Tsjakamaram. REEDE, Hort. Malabar. P. III. pag. 17. tab. 26, 27, 28.

Saccus arboreus major. Nanca. RUMPH. Herb. Amboin. P. I. p. 104. tab. 30.

Saccus arboreus minor. Tsjampadàha. RUMPH. Herb. Amboin. P. I. pag. 107. tab. 31.

Jaca. ZANON. Hist. Stirp. rarior. pag. 127. tab. 90, 91.

Radermachia integra. Ego Act. Stockholm. anno 1776, pag. 254.

Malais. Tsjampedaha, Nanca, Nanca bubur.

Portugallis. Jacca sive Jaccas.

Hollandis. Syrfack, Scoorfack, Schrootfack, et Broodboom proprie.

Crescit

Creſcit ſponte in inſulis Moluccis, Ambon, Banda, aliis; in Java parcius, Sumatra, Ceilona vulgaris, Coromandelia, Tranquebaria, inque promontorio Comorin.

Floret fructuſque bis in annum, primis octo menſibus, ſenſim profert.

Caulis arboreus, erectus, craſſitie humana, quinqueorgyalis et ultra. *Rami* alterni, patentes, ramuloſi. *Ramuli* alterni, iterum ramuloſi, villis longis rigidis hirſuti.

Folia alterna, petiolata, ovato-oblonga, obtuſa cum acumine obtuſo, obſolete ſerrata, indiviſa, nervoſa; ſupra læte viridia, læviſſima; ſubtus palliodiora, pilis rigidis hirſuta; patentia, ſpithamæa. *Juniora* evidenter dentata ſunt, qui dentes in adultis evaneſcunt. *Petiolus* ſubtriqueter, glaber, pollicaris. *Stipulæ* ut in priori. Raro folium unum alterumve inciſum.

Flores maſculi et feminei in caule vel ramo eodem diſtincti. *Pedunculus* vel ſimplex vel ramoſus, pendulus, pollicem craſſus, pedalis. *Pedicelli* tres, quinque vel plures, digitum craſſi longique.

Spadix digitalis, erecto-patens.

Pericarpium magnitudine inſigni, pondere 30 librarum et ultra, fertile, adeo ut omnium baccarum facile maxima hæc ſit. *Semina* magnitudine tripla et quadrupla Amygdalarum, ſæpe 200 uſque 300, ovato-oblonga,

altera

altera extremitate acuta, altera obtusa, parum complanatis lateribus.

Observationes in genere circa utramque speciem.

Arbor tota, rami fructusque abundant succo lacteo albo, fibroso et tenacissimo, qui sine oleo ablui non potest.

Crusta extima fructuum coriacea est, tuberculis undique hispida et muricata; hac ablata semina in conspectum veniunt, quae carne et variis tunicis circumvolvuntur. Primum inter semina deteguntur lamellae albae, carnosae, tenues, lineares, semina distinguentes. Deinde singulum semen circumvolvitur tunica carnosa, striata, immatura alba, matura flava, sapida, molli. Postea observantur duae tunicae tenues et seminibus magis propriae, quarum exterior alba et siccatione secedens, interior brunnea; hae duae conservari debent in seminibus plantandis. Media columna in fructibus parvis valde crassa est, quo vero magis fructus accrescunt et maturantur, eo haec tenuior evadit.

Utriusque speciei fructus, dum maturus est, odoris paululum ingrati et fere nauseosi est, licet interiora delicata sint.

Si pluviæ cadant tempore floreſcentiæ, pericarpia parva putrefacta decidunt et parca tunc erit meſſis; ſi vero ætatem quindecim dierum dudum attigerint, pluviæ vix amplius nocent.

S. inciſum longe vulgarius eſt inſula Java, *macrocarpon* vero in Ceilona, ubi illi præfertur tam ob gratiorem ſaporem, quam ob inſignem ſuam magnitudinem.

S. macrocarpon propagatur ſeminibus, quæ facile excreſcunt; *inciſum* vero, quod ſterile eſt, radicibus ſub ipſa ſuperficie terræ plantatis. *S. inciſum* optime colitur tam in humo arena mixta quam in terra argillacea; *macrocarpon* vero amat argillam humo mixtam.

Flores fructuſque primos *S. inciſum* profert tertio quartove anno, *macrocarpon* vero anno quinto vel ſexto: utrumque perennare dicitur annos 100 uſque 150, ſemper fructiferum. *S. inciſum* fructus ſuos profert in ipſis extremitatibus ramulorum, *macrocarpon* vero in ipſo trunco et majoribus ramis.

Obſervationes circa S. inciſum.

Species hæc longe communiſſima eſt in orientalibus regionibus Indiæ Orientalis. In Ceilonam, Mallabariam, et Coromandeliam allata fuit et radicibus propagata. In inſulam Ceilonam primum ab incolis Maldivicarum inſularum, qui quotannis naviculis adveniunt, annis 1727

vel

vel 1728 allatæ fuerunt radices et plantatæ intra Castellum juxta templum in horto domus proximæ. Duas has arbores proceras ipse vidi, e quibus in Ceilona plurimæ aliæ, magna copia propagatæ sunt. Ob hanc rationem hodie adhuc ab Hollandis arbor Maldivica (*Maldivische Syrsack*) appellatur.

Radices plantatæ primum post spatium duarum, trium vel plurium mensium mihi inceperunt prima germina emittere, licet sub sirio ardente et intra tropicos plerumque navigans qualibet fere vespera aqua non pauca irrogaverim.

Semina hujus magnitude dupla pisi circumdantur carne tenuissima et delicata, sed sterilia sunt nec plantata excrescere volunt.

Observationes circa S. macrocarpon.

Radices amant superficiem terræ sique alte sub terra impositæ sepeliuntur, arbor emori dicitur, præcipue ab humo pingui.

Dum fructus maturare incipiunt, comeduntur a Sciuro palmarum atque Corvis, si non ab horum insidiis præservantur. Sciurus palmarum primus plerumque est qui vulnus infligit, et apertura ab illo facta adveniunt Corvi, illum fugantes quodque supereſt fructus comedentes.

Fructus varios trunci et ramorum maturos ponderavi, qui plerumque 30 et 35 librarum pondere fuerunt.

Narratum mihi fuit a variis fide dignis, quod ipſæ radices infra ſuperficiem terræ interdum fructus proferant, qui ſub maturatione terram divellunt et adeo ponderoſi ſæpe evadunt, ut a duobus ſervis portari debeant ſinguli, hique fructus maxime delicati æſtimantur.

Accidit interdum, ut fortioribus ventibus dejiciantur fructus ponderoſi, dum valde grandes e tenui et non ſatis forti pedunculo dependent. Dum valde grandes evadunt fructus et aliquot eidem inſident pedunculo ut tres, quinque vel plures, tunc plerumque corbes ex foliis cocos confectæ ſubponuntur, ne ab hoſtibus ſuis ſupra memoratis lædantur, nec pericarpia adhuc immatura, præ pondere rupto pedunculo, decidant et ne noceant invidi oculi, unde fructus decidunt, ut ſuperſtitioſe credunt.

Duobus vel ſaltem tribus menſibus fructus ſuos maturat hæc ſpecies, dumque primi fructus maturi ſunt, iterum florere incipit.

Ceilonenſes triplici nomine inſigniunt hos fructus, prout illis utuntur varia ætate et prout magis vel minus maturi ſunt. *Pollos* audiunt, dum adhuc parvi magnitudine ovi Struthionis, et immaturi ſunt, ætate dimidiæ vel integræ menſis; hoc quoque nomen tribuitur fructibus *S. inciſi* in genere. *Herreli*, dum ſemimaturi ſunt, magnitudine cocos unius cum dimidia. Caro ſeminum

tunc adhuc alba, lacteſcens eſt, nec ſine præparatione edulis. *Warreka*, dum plenam fructus maturitatem attigerunt; caro ſeminum mollis, dulcis, flava et ſine prægreſſa præparatione edulis et delicatiſſima eſt, licet dulcedo aliquantum nauſeoſa ſit. Maturitatem minorem vel majorem concludimus ex mollitie et ſono, quem digitis pulſatus fructus plus vel minus obtuſum edit. Pollos diſſectus in aëre rubeſcit et adſtringens eſt, ſed coctus iterum albeſcit. Diſtinguunt quoque nonnulli fructus in duplicem varietatem, ſcilicet nucleis duris et mollibus: e mollibus conficitur *Fios*.

Hæc ſpecies in Ceilona vulgatiſſima eſt et ſpontanea, Cingallis pauperibus, qui nihil vel parum oryzæ poſſident, maxime neceſſaria et inſignis utilitatis, dum fructum cum pauxillo raſuræ Cocos vel cum raſura Cocos et oryzæ parum comedunt.

Mos obtinuit in Ceilona ſemina hujus cum humo et aqua mixta in diſciſſo Cocos externo involucro ſerere, ut germinent, antequam in hortis plantentur.

Licet hæc ſpecies per totum fere Orientem creſcat, nullibi tamen locorum vel majores vel ponderoſiores fructus vel delicatiores profert, quam quidem in inſula Ceilona et quibuſdam Mallabariæ regionibus, ut ipſe non modo obſervavi oculatus teſtis, ſed et in antiquis peregrinatorum ſcriptis adnotatum inveni.

USUS.

usus. *Lignum* paucorum annorum, nimis lacteſcens adhuc, ullius vix uſus eſt. Cum vero arbor 20 vel 30 et 40 annos attigerit, adhiberi poteſt lignum pro vario uſu in conſtruendis domibus, pro laminis, trabibus; in conſtruendis naviculis, ubi injurias aquæ et aeris 30 annos et ultra perdurat; in fabricandis ſellis, ſcamnis, ciſtis, cæteris.

Lac arboris inſervit pro capiendis pſittaccis et aliis avibus, dum inſtar viſci inungitur arborum ramis.

Fructus autem præprimis eſt, qui generi humano tantæ utilitatis eſt, vel ſterilis vel ſeminibus farctus.

Bacca, dum omnino matura eſt, ſcilicet *S. macrocarpi*, carnem habet ſemina obveſtientem, mollem, flavam, dulcem et delicatam, quæ ſine ulla præparatione prægreſſa vel ſola editur vel cum oryzæ pauxillo, et optime ſapit. Hæc caro in duas vel tres partes diſciſſa cum oleo recenti Cocos aſſata quoque comeditur.

Bacca utriuſque ſpeciei, dum adhuc immatura eſt, id eſt Pollos et Herreli, variis modis præparata nutrimento inſervit. Ingreditur præparationem Currii Caldu, Currii ſeco, Niembelæ, Lixationis cum lardo, Pollos tjundido, fricadellarum, Pollos aſſati, Empade, Pei et hinc, in futuros uſus, exſiccatur fructus.

Pauperes Cingalli ſequenti modo fructus e *S. inciſo*

cisso et e *S. macrocarpo* exemtis seminibus, præparatos adhibent. Omnia cultro in tenuissimas et minutissimas partes concindunt, cumque Cocos raspato *a* supra lapidem contrita vel sola comedunt vel cum additis Capsicis, sale et Allio. Mos quoque Hollandis in Indiis est, Pollos utriusque speciei in tenues lamellas concisum cumque oleo fortiter assatum comedere, juxta potum Theæ. Hæ lamellæ tenues aliquando etiam lixantur cum carne et jusculo gallinarum.

Nuclei vel cum tunica sua carnosa, vel cum toto fructu, vel soli pro nutrimento adhibentur ut in ferculis variis, fructu cocto, Pollos tjundido, confectione seminum, Fios placentulisque. *Nuclei* mundati etiam soli eduntur tam a ditioribus quam a pauperibus, cocti vel assati. Cocti plerumque a pauperioribus cum Cocos raspato et sale comeduntur. Ditiores interdum hisce in quatuor plerumque partes concisis et passulis mixtis vel porcos juniores vel anseres assatos, loco Castaneárum, implere sciunt. Usus, quos habent medicales nuclei, tanti quidem non sunt; nihilo tamen minus Javani exinde cum lympha Cocos emulsionem parant, quam Diarrhœis laborantibus propinant.

Omnibus itaque prioribus, quæ homini alicujus utilitatis esse possunt, pro nutrimento et alimento adhibitis, cætera tamen ut inutilia non abjiciuntur, ut cortex fructus externus,

externus, lamellæ paleaceæ, quæ ſemina circumſtant, tunicæ nucleorum et columna media; ſed hæc in minutas partes conſciſſa et cocta, ſuibus, qui avide devorant, apponuntur.

E columna media diſciſſa, parum cocta et in aceto macerata Atjar conficiunt. Atjar (Pickles) vocant omne, quod aceto forti conſervatur et pro condimento ciborum inſervit. Ex pollos quoque Atjar fit, dum antea decorticatus et coctus aceto inditur.

Deſcriptio fercularum, quas e fructibus præparant Hollandi *in* India *Orientali, præſertim in inſula* Ceilona.

1. *Currii Caldu.* Rc. tenuium lamellarum Pollos ℳ ij. Coque parum cum aqua et radice Curcumæ ut flaveſcant. Adde Piſcis ſiccati (Comlimas dicti, e Maldivicis inſulis adducti) et contuſi ℙ ij. ſi velis; lactis Cocos ſecundi ℔ ij. Coque iterum tenui igne per ſemihoram, aliquando agitando.

2. *Curii ſeco.* Rc. tenuium lamellarum Pollos ℳ ij. Coque cum aqua et radice Curcumæ parum, ut flaveſcant. Adde Cocos uſtæ contritæ ſimul cum ſeminibus Coriandri et piperis, cortice cinnamomi, mace et aquæ ſalſæ parum: lardi ſaliti cocti et in parva fruſta conciſi q. vis: aquæ, in qua coctum fuit lardum, parum.

Omnia

Omnia haec bene miscuntur et adduntur lactis Cocos cochlearia aliquot. Coque per dimidiam horam. Postea alio vase assatur butyro herba allii concisa, priora omnia adduntur beneque miscuntur. Additur porro succus duarum limonum, miscetur optime et paululum aquæ salsæ ad gratum saporem additur. Quidam addunt unicum Capsicum et duos allii bulbos.

3. *Pollos Tjundido.* Rc. Pollos vel Herreli cum vel sine nucleis tenue consciforum q. vis. Coque primum cum aqua et radice Curcumæ. Adde Cocos raspatæ cum Capsico, allii bulbis concisis et sale M ij. Coque iterum leni igne massam fere siccam, sæpe agitando et interdum vase clauso. Coctione per dimidiam horam peracta, paratum est.

4. *Currii Castaneum.* Rc. Nucleorum a tunicis mundatorum et in longis angustis lamellis conciforum circiter M ij. Coque parum cum aqua et radice Curcumæ, ut flavescant. Adde, si velis, Piscis Comlimas dicti P j: lactis Cocos secundi ℔ß – 1: Allii herbæ concisæ parum. Coque vase clauso per dimidiam horam leni igne, interdum agitando. Adde lactis Cocos primi paululum. Coque iterum, continuo agitando, per $\frac{1}{4}$ horæ partem.

5. *Niembela.* Rc. Pollos vel Pollos et Herreli valde grosse conciforum q. vis. Adde aquæ salitæ et allii foliorum

conciforum parum: lactis Cocos primi lbj. Piſcis (Comlimas dicti) contuſi ℥ ij. Coque leni igne, ſæpe agitando, per dimidiam horam.

6. *Lixatus cum lardo fructus.* Rc. Pollos et Herreli groſſe conciſorum q. v. Adde aquæ ſalitæ et allii herbæ conciſæ parum: lardi falſi cocti et in fruſta conciſi atque aqua frigida abluti q. vis: aquæ, in qua lardum coctum fuit, paululum: macis et fruſtulorum Cinnamomi, herbæ allii conciſæ et cinnamomi pulveriſati *aa* ℥ ij. Coque per quadrantem horæ, aliquando quaſſando.

7. *Fructus coctus.* Rc. nucleorum cum carne et tunicis maturorum, inque duas vel tres partes conciſorum q. v. Coque cum aqua et radice Curcumæ q. s. addendo ſub coctione paululum ſalis. Ita coctus comeditur a pauperibus, cum raſura nuclei Cocos; ditiores addunt Capſicum et piſces ſiccatos.

8. *Lamellæ Pollos aſſatæ.* Rc. lamellarum tenuium Pollos coctarum q. vis: lactis Cocos lb ß: farinæ ℳ ij. Hiſce mixtis involve lamellas et in vaſe plano aſſa cum olei Cocos recentis magna copia.

9. *Empade.* Rc. lamellarum Pollos ut ſupra aſſatarum q. v. Adde herbæ allii tenue conciſæ, lactis Cocos (Comlimas) piſcis ſiccati, bulborum allii conciſorum, et pulveris Cinnamomi. Coque vaſe plano, leni igne,

continuo cum cochleari lac Cocos super affundendo. Per quadrantem horæ ita coctis adde allii bulborum tenue conciſorum et cum butyro aſſatorum, quantum lubet.

10. *Fricadellæ ex Pollos.* Rc. Pollos cocti et in pultem redacti ℳ ij; allii herbæ conciſæ, Cinnamomi, Piperis, nucis moſchatæ, ſalis, panis biſcocti *aa* pulveriſatorum q. vis; vitellum ovi unicum. Miſce optime fiantque inde boli, obvolvendi albumine ovi, ut melius cohæreant. Poſtea aſſentur butyro ſolo vel oleo Cocos et butyro ſimul, uſque dum rufeſcant. Paratis ſic bolis ſuper affunditur embemma e butyro, pulvere cinnamomi, pipere, ſale et ſucco limonum confectum.

11. *Confectio ſeminum et carnoſæ ejus tunicæ.* Rc. Tunicarum carnoſarum ſeminum in duas vel tres partes conciſarum q. v. Aſſa cum oleo expreſſo recenti Cocos. Oleum deterge cum linteo vel pone aſſatas tunicas in cribro, ut defluat oleum. Poſtea obvolve ſyrupo ſacchari condenſato, ſicca et lagenis bene obturatis conſerva. Per tres menſes ita conſervari poſſunt et eduntur juxta potum Theæ.

Rc. Seminum a tunicis mundatorum et integrorum. Cum oleo aſſata involve ſyrupo ſacchari, ſicca et ut priores conſerva in eundem uſum. Si ſemina aſſata

cum ipso syrupo in lagenis obturatis includuntur, per dimidium annum conservari possunt.

12. *Fios.* Rc. seminum maturorum q. v. involve pulti e lacte Cocos, farina et vitello ovi confectæ, Assa cum oleo recenter expresso Cocos.

13. *Placentulæ.* Rc. Vespere Syri dicti (i. e. aceti e succo Cocos fermentato): farinæ q. v. unde fiat bolus. Altero mane, nocte peracta fermentatione, adde lactis Cocos, farinæ seminum siccatorum q. s. et ovorum vitella pro lubitu. Fiant modo consueto placentulæ.

14. *Pei* sive *Jambal.* Rc. Pollos cocti, seminum sinapis *aa* ℳ ij, radicis Curcumæ q. v. Omnia seorsim in pultem redacta, optime misceantur cum paululum aceti. Adhibetur cum aliis cibis, cum piscibus et cum oryzâ quandoque sola, ob gratum saporem, præsertim pauperibus. Quidam his quoque addunt Capsicum, zingiber, allium et sal commune, ut priora in pultem redacta.

15. *Siccatio fructuum.* Rc. fructus semimaturos vel integros vel discissos exemta carne. Coque parum et postea sicca optime insolatione, et ita serva vel suspensos in Camino vel alio sicco loco. Hac ratione per annum conservari possunt. Pauperes ita siccatos cum Cocos rasura comedunt vel etiam coctos. Dum aliquando magna copia colligunt fructus, ita illos siccatos in futuros usus conservant.

Observatione,

Observationes circa fercula præparanda.

Currii, quod communissimum Indianis est ferculum, jusculum quoddam audit, cui additur radix Curcumæ ut flavum induat colorem, et capsicum, ut igneus plus minus evadat sapor, præter pauca alia aromata. Ejusmodi jusculum fit e jusculo carnium ut gallinarum, anserum, anatum, ovium, pisciumque; item ex emulsione nucleorum Cocos, cum carnibus supra dictis vel cum carnibus et variis simul fructibus concisis ut melonibus, bilimbing (Averrhoa Bilimbi) aliis, vel etiam cum hisce et aliis fructibus absque carne. Dum jusculum adeo paucum est, ut fere siccum, vocatur *Currii seco.*

Omne, quod Currii audit, ingredi debet radix Curcumæ, ut color evadat flavus et ad saporem acrem plus minus igneum, Capsicum et alia aromata, pro cujusque lubitu, adduntur ut Currii Caldu, Currii seco, Currii castaneo et Pollos tjundido.

Radix Curcumæ, quæ communi nomine in Indiis appellatur *Borriborri*, et ad finem Currii additur, sequentem in modum præparatur. Primo aqua maceratur ut mollis fiat, dein supra lapidem planum cum pauxillo aquæ conteritur in pultem sinapis densioris consistentiæ. Lapis, plerumque *lapis Borriborri* dictus, planus est, plus

vel minus magnus, cum pistillo lapideo, ea longitudine ac ipsa latitudo lapis, cylindrico, altera extremitate rotundato, altera transverso. Super hunc lapidem in pultem teruntur non modo radix Curcumæ sed etiam pollos coctus, sinapis, Capsicum, allium, Cocos rasparum, oryza tosta et alia aromata, &c.

Curryi seco varia requirit, quæ antequam in pultem redigantur, tosta esse debent ut Cocos, semina Coriandri, cortex Cinnamomi et, ut quidam amant, oryza, piper, feniculum et cetera. Hæc singula vel separatim vel aliquot simul mixta in vase terrestri super ignem, perpetuo agitando comburuntur, donec brunnea evadant. Tunc super lapidem cum adjecta aqua in pultem fere solidam rediguntur.

Fructus, prima et secunda sua ætate, id est, Pollos et Herreli, ante præparationem decorticantur et inciduntur. In octo plerumque partes fructus primum inciduntur et singulum frustum separatim decorticatur. Deinde singula pars inciditur in frusta vel lamellas majores vel minores, crassiores vel tenuiores pro usu diverso. Incisio hæc adeo peragitur: cultri manubrium firmum tenetur inter hallucem et proximum pedis digitum, ita ut acies superiora spectet et sub medio posito ligno apex elevetur, infra quem fructus inter ambas manus retentus incidítur. Incidentes tunc sedere debent in ipso solo vel pavimento

substrato

ſubſtrato tapete, vel in ſcamno humillimo, ne altius ſedentes dorſi dolorem contrahant. Hoc modo conſcinduntur non modo fructus completi, et fructus abſque nucleis; ſed etiam nuclei, vel cum tunica circumvolvente et tunicis, vel ab eis mundati. Hæc eſt methodus plerorumque Indianorum et quorundam Europæorum in Indiis degentium; ſed hæc quoque inciſio ſupra menſam cultro inſtitui poteſt. Conciſa omnia aqua purificantur ab arena et aliis adhærentibus immunditiis.

Nuclei a tunica carnoſa ſeparati a ceteris tunicis quoque mundantur. Exteriorem albam tunicam ſingulari modo ſeparant, ſcilicet nucleum perpendiculariter ſupra lapidem collocant et lapillo extremitatem ſupremam ſeu apicem pulſant, uſque dum diſrumpitur tunica et facile digitis ſeparari poteſt. Intimam brunneam tunicam cultro radendo eximunt.

Lac ſeu emulſio nucleorum Cocos, eodem, quo aliæ emulſiones modo, præparatur. Proprio primum inſtrumento nucleus raſpatur, aqua frigida additur, raſura inter digitos premitur et emulſio decolatur. Hoc *lac primum* vocant cum fortius ſit; ſi vero iterum raſuræ aqua ſuper infunditur et expreſſio fit, obtinetur *lac* ita dictum *ſecundum*, quod dilutius eſt.

Proportionem

Proportionem ingredientium circum circa indicavi, secundum quam ipse vidi hæc fercula præparari; sed illa tamen pro cujusvis gustu arbitraria est, sic ut hoc vel illud secundum placitum augeri possit vel diminui.

XXXI. *A Second Paper concerning ſome Barometrical Meaſures in the Mines of the* Hartz. *By Mr.* John Andrew De Luc, *F. R. S.*

Read June 10, 1779.

THE Royal Society having been pleaſed to accept the communication which I had the honour to make to it two years ſince of certain barometrical experiments in the mines of the Hartz, I take the liberty of communicating to it ſome others of them, which I made the laſt year in one of the deepeſt of the mines in thoſe mountains, named the *Deep St. John*.

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Second Mémoire ſur des Meſures Barométriques dans les Mines du Hartz. *Par Monſ.* De Luc, *F. R. S.*

LA Société Royale ayant bien voulu agréer la communication que j'eus l'honneur de lui faire il y a deux ans de quelques expériences du baromètre dans les mines du Hartz, je prends la liberté de lui en communiquer d'autres, que je fis l'année derniere dans une des mines les plus profondes de ces montagnes, nommée le *Profond St. Jean*.

It was on the 5th of July that I went down into it. I had been informed that I might not perhaps be able to arrive at the loweſt galleries becauſe of the foul air which had been there all the preceding days. Some miners therefore went before to examine how far we might go down without danger. Mr. USLAR, who had been ſo kind as to accompany me to the mines of Ramelſberg, was alſo pleaſed to be of this party, as well as Mr. FREDERIC, one of the officers of the mines, a gentleman well ſkilled in ſubterraneous geometry. And whilſt we were under ground Mr. MAYER, one of the clerks of the office of the mines, obſerved his barometer above ground every quarter of an hour.

It had been fine weather for ſome days, and that day it was particularly ſo; and Mr. MAYER perceived no variation

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Ce fut le 5e Juillet que j'y deſcendis. On me prévint que je ne pourrois peut-être pas arriver juſqu'aux galeries les plus baſſes à cauſe des mouffettes, dont il y avoit eu tous les jours précédens. Quelques mineurs prirent donc les devants pour examiner juſqu'à où nous pourrions deſcendre ſans danger. Monſ. DE USLAR, qui avoit eu la bonté de me conduire aux mines du Ramelſberg, voulut bien encore être de cette partie, de même que Mr. FREDERIC, l'un des officiers mineurs, très au fait de la géométrie ſouterraine. Et tandis que nous fûmes dans l'intérieur de la terre, Mr. MAYER, l'un des officiers du bureau des mines, obſerva ſon baromètre chaque quart d'heure à l'extérieur.

Il faiſoit fort beau depuis quelques jours, et ce jour là en particulier; et Mr. MAYER n'apperçut aucune variation dans ſon baromètre de 4 h. à 9 h. du ſoir:

riation in his barometer from four to nine o'clock at night, which, at that height, indicated the greateſt ſtability in the ſtate of the air in all other reſpects, excepting for the ordinary variations of the diurnal heat. Had it been on the Plain, the immobility of the barometer, in that part of the day, would have indicated that it had a tendency to deſcend. I ſhall not dwell on the cauſe of this difference, which reſults from that of the heights; I have explained it in my work on the atmoſphere.

It was $4\frac{1}{4}$ h. when I made, at the entrance of the mine, the firſt obſervation of the barometer and of the thermometer which accompanies it; and at the bottom of the firſt ladder, that of the detached thermometer. Continuing to deſcend we arrived at $5\frac{1}{2}$ h. towards the middle of the depth of the mine, near the beginning of a gallery

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ce qui, à cette hauteur, marquoit la plus grande ſtabilité dans l'état de l'air, à tout autre égard que pour les variations ordinaires de la chaleur diurne. A la Plaine, l'immobilité du Baromètre dans cette partie du jour, eût marqué qu'il tendoit à deſcendre. Je ne m'arrêterai pas à la cauſe de cette différence, qui reſulte de celle des hauteurs; je l'ai expliquée dans mon ouvrage ſur l'atmoſphère.

Il étoit $4\frac{1}{4}$ h. lorſque je fis à l'entrée de la mine la première obſervation du baromètre et du thermométre qui l'accompagne; et au bas de la première échelle celle du thermométre détaché. Continuant à deſcendre, nous arrivâmes à $5\frac{1}{2}$ h. vers le milieu de la profondeur de la mine, auprès du commencement d'une

a gallery named the George, where I made an obſervation at $5\frac{3}{4}$ h.

The miners who had gone before us, having come back from the bottom of the mine, reported that the vapours were diſſipated, and that the air was every where good. So we continued to deſcend to the place where, for want of a ſufficiently deep draining gallery, the water is obliged to be left, although by ſome extraordinary efforts they had formerly ſunk the ſhaft ten or twelve fathoms deeper. This point is at the level of the eleventh gallery. I obſerved there at $7\frac{1}{2}$ h. The air was perfectly wholeſome, only a little warmer than at the top of the ſhaft.

In aſcending again I repeated the obſervation at $8\frac{1}{2}$ h. near the George gallery; the barometer ſtood then one eighth of a line higher than the firſt time. On return-

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galerie nommée George, où je fis une obſervation à $5\frac{3}{4}$ h.

Les mineurs qui nous avoient précédés, étant alors de retour du fond de la mine, nous rapportérent que les mouffettes étoient diſſipées, et que l'air étoit bon partout. Ainſi nous continuâmes à deſcendre juſqu'au point où, manque d'une galerie d'écoulement aſſez profonde, on eſt obligé de laiſſer l'eau; quoique, par des efforts, on ait approfondi autre fois le puits de 10 à 12 toiſes de plus. Ce point eſt à niveau de l'onziéme galerie. J'y fis l'obſervation à $7\frac{3}{4}$ h. L'air y étoit parfaitement ſain; ſeulement il étoit un peu plus chaud que dans le haut du puits.

En remontant, je réiterai l'obſervation à $8\frac{1}{2}$ h. auprès de la galerie George; le baromètre s'y tint plus haut d' $\frac{1}{8}$ de ligne que la première fois. De retour à

9 heure

ing to the top of the mine at nine o'clock I observed there again, and found the barometer one sixteenth of a line higher than when I had entered.

Combining these observations, which are set down at the end of this paper, I found that the George gallery was 127·15 lachters, or Hartz fathoms, below the entrance of the mine; and that the whole depth was 215·86 lachters, which makes 801 English feet for the first depth, and 1359 for the latter.

Mr. USLAR, having taken a memorandum of the places where I had made my observations, afterwards gave me the correspondent geometrical measures. The depth of the George gallery he found to be 127·87 lachters, that is, only four feet more than what I had found by the barometer; but I was much surprised to see that, in the whole

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9 heures au haut de la mine, j'y observai encore, et je trouvai le baromètre plus haut d' $\frac{1}{16}$ de ligne que lorsque j'étois entré.

Combinant ces observations, dont les détails sont à la fin de ce mémoire, je trouvai que la galerie George étoit abaissée de 127,15 lachters, ou toises du Hartz, au dessous de l'entrée de la mine; et que la totalité de la profondeur étoit de 215,86 lachters. Ce qui fait 801 pieds Anglois pour la prémière profondeur, et 1359 pour la derniere.

Mr. DE USLAR ayant pris note des lieux où j'avois fait mes observations, me donna ensuite les mesures géométriques correspondentes. La profondeur de la galerie George se trouvoit être de 127,17 lachters c'est à dire plus grande seulement de 4 pieds que celle que j'avois trouvée par le baromètre: mais je fus bien

whole depth, the geometric meaſure exceeded mine by 110 feet.

At firſt I ſuſpected an error in my obſervation, and I reſolved to go down into the mine again. But afterwards, from a compariſon of the ſucceſſive obſervations, in which the particular conſtruction of the ſcale of my barometer would have ſhewn the error, I ſaw clearly that my obſervation was well made. I ſhall not in this place explain the nature of that examination, for fear of being too long; but it is eaſily deduced from the conſtruction of that ſcale.

I had no longer reaſon to think, that the barometric meaſure could in itſelf differ ſo much from the geometric meaſure, ſince they agreed ſo well in the middle of the ſhaft. There remained nothing to be ſuſpected but the geo-

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ſurpris de voir que ſur la profondeur totale, la meſure géométrique excédoit la mienne de 110 pieds.

Au premier moment je ſoupçonnai une erreur dans mon obſervation, et je me réſolus de retourner dans la mine. Mais enſuite, par une comparaiſon des obſervations ſucceſſives, dans laquelle la conſtruction particuliere de l'echelle de mon baromètre eût fait appercevoir cette erreur, je vis ſans aucun doute que mon obſervation étoit bien notée. Je n'expliquerai pas ici la nature de cet examen, de peur d'être trop long; mais on le conclura aiſement de la conſtruction de cette echelle.

Je n'avois point de raiſon non plus de croire que la meſure barométrique pût en elle-même s'écarter autant de la meſure géométrique; puis qu'elles s'accordoient ſi bien dans le milieu du puits. Il ne reſtoit donc à ſuſpecter que la note qui

geometric meaſure which had been given me, and I requeſted Baron DE REDEN to cauſe it to be examined. The perſon whom he charged with that care was his nephew, who is very ſkilful in every thing concerning the mines. He examined with Mr. RAUSCH, the chief geometer, the meaſure which had been given me; and they found, that in writing the depths of the differents parts of the mine, the ſum of which ought to give the whole depth, one of theſe parts had been twice put down, ſo that the true depth was only 215½ lachters: it became then two feet leſs than that which reſulted from my obſervations.

Thus the barometric meaſurement gave four feet leſs than the geometrical meaſure for the former of the depths, and two feet more for the latter.

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qui m'avoit été donnée de la meſure géométrique; et je priai M. le Baron DE REDEN de la faire examiner. La perſonne qu'il chargea de ce ſoin, fut ſon neveu, qui eſt très éclairé dans tout ce qui concerne les mines. Il vérifia donc cette note avec Mr. RAUSCH le géométre en chef; et ils trouvérent, qu'en prenant en détail les profondeurs des différentes parties de la mine, dont la ſomme devoit donner la profondeur totale, on avoit poſé deux fois une de ces parties; deſorte que la vraie profondeur n'étoit que 215½ lachters. Elle étoit donc alors moindre de 2 pieds, que celle qui reſultoit de mes obſervations.

Ainſi la meſure barométrique avoit donné 4 pieds de moins que la meſure géométrique pour la premiére des profondeurs, et 2 pieds de plus pour la derniere.

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I was at firſt as much ſurprized as ſatisfied with this exactneſs, which I did not expect notwithſtanding my former experience; and I ſhould not have been ſurprized to find two or three fathoms difference either way. But, reflecting on it afterwards, I perceived that there ought to be more certainty in the barometric meaſure for the depth of mines, than for the height of mountains; and that for the two following reaſons.

Firſt, that in making theſe obſervations along the ſhafts of mines we go but little from the ſame vertical column, and by that we know with certainty the weight which compreſſes the part which we meaſure: whereas in mountains, even the ſteepeſt, the obſervations are made in columns of air which are far enough from one another to be liable to ſenſible differences on that account; and

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Je fus d'abord auſſi ſurpris que content de cette exactitude, à laquelle je ne m'attendois point malgré mes précédentes expériences; et je n'aurois pas été étonné de trouver 2 au 3 toiſes d'écart dans l'un ou l'autre ſens. Cependant, y réfléchiſſant enſuite, j'ai compris qu'il doit y avoir plus de ſureté dans la meſure barométrique pour la profondeur des mines, que pour la hauteur des montagnes; et cela par les deux conſidérations ſuivantes.

La premiére, qu'en faiſant ces obſervations le long des puits des mines, on s'écarte peu d'une même colonne verticale; et l'on connoit ainſi ſurement le poids qui comprime la partie qu'on meſure. Aulieu que dans les montagnes, même les plus rapides, les obſervations ſe font dans des colonnes d'air aſſez éloignées l'une de l'autre, pour qu'il puiſſe y avoir des différences ſenſibles à cet égard; et que

and that if, for example, the higher barometer were moved horizontally as far as the column which reſts upon the ſtation of the lower one, it would not ſtand at the ſame point as when it was on the mountain; which, however, is ſuppoſed in the formula.

The other circumſtance reſpects the homogeneity of the air. In the open air a thouſand lateral cauſes may introduce into the portion of the column which we meaſure ſome layers of air different from thoſe of the two extremities, were it only in point of heat. And in that reſpect the correſpondent obſervations neareſt the foot of the mountain are not always the moſt certain, becauſe of the reflexions of the ſun, and even of the winds which carry the vapours; and the reſult muſt be that we very often find the heights too ſmall. I ſhall not ſtop to prove

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que par exemple, ſi le baromètre ſupérieur étoit avancé horizontalement juſqu'à la colonne qui pèſe ſur la ſtation inférieure, il ne ſe tint pas au même point que ſur la montagne; ce qui néantmoins eſt ſuppoſé par la formule.

L'autre circonſtance regarde l'homogénéité de l'air. En plein air, mille cauſes latérales peuvent introduire dans la portion de colonne qu'on meſure, des couches d'air différentes de celles des deux extrémités; ne fût-ce que différemment chaudes. Et à cet égard les obſervations correſpondentes les plus près du pied des montagnes ne ſont pas toûjours les plus ſûres à cauſe des réflexions du Soleil, et de celles des vents qui charient les vapeurs: et il doit en réſulter très ſouvent qu'on trouve les hauteurs trop petites. Je ne m'arrêterai pas à le montrer.

prove it. In mines, on the contrary, the air being inclоſed as in a canal, where it is always in motion, it continually intermixes and becomes more homogeneous: it contracts there a temperature which is more uniform or more gradually different, ſo that we can more certainly fix the mean ſtate of it.

On theſe two accounts, therefore, barometric meaſures ought to be more regular in moſt mines than on the ſides of mountains; becauſe the obſervations are made nearly at the extremities of one and the ſame column, and becauſe the whole column is more nearly ſimilar to its extremities; conſequently we are ſurer that the particular caſe is not an exception to the rules. Now it has happened, that in all the obſervations which I have made in the Hartz mines, my rules, drawn from a mean among my obſervations in open

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trer. Dans les mines au contraire, l'air étant renfermé comme dans un canal, où il eſt toûjours en mouvement, ſe mêle ſans ceſſe et ſe rend plus homogène: ſurtout il y contracte une température plus uniforme ou plus graduellement différente; enſorte qu'on peut plus ſurement en fixer le terme moyen.

Les meſures barométriques doivent donc, par ces deux conſidérations, être plus régulières dans la plûpart des mines, que ſur les pentes des montagnes; c'eſt à dire parce que les obſervations ſont faites à peu prés aux extrémités de la même colonne, et que la totalité de la colonne a des rapports plus reguliers avec ſes extrémités. Ainſi l'on eſt plus ſûr que le cas n'eſt pas une exception aux régles. Or il ſe trouve en même tems, que dans toutes les obſervations, que j'ai faites dans les mines du Hartz, mes régles, tirées du milieu entre mes obſervations en

plein

open air, have agreed with the geometric meaſures, which ſeems to indicate that the uſual ſtate of the air of the mines is a mean among the different ſtates of the exterior air as found in the whole of my obſervations; the almoſt conſtant ſtate of the heat in the mines may be the principal cauſe of it.

As to the geometric meaſures themſelves, I might here give ſome very intereſting proofs of their exactneſs by the deſcription of the aſtoniſhing works which are undertaken in conſequence of them; but I ſhould be afraid of being too tedious. And, beſides, we may depend on the miners for the exactneſs of thoſe meaſures, they being more intereſted in them than we are. I ſhall, therefore, conclude this paper with ſome general remarks on barometrical meaſurement.

So

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plein air, s'y ſont accordées avec les meſures géométriques; ce qui paroit indiquer, que l'état habituel de l'air des mines, eſt moyen entre les divers états où il s'eſt trouvé dans la totalité de mes expériences; et l'état preſque conſtant des mines quant à la chaleur, pourroit bien en être la principale cauſe.

Quant aux meſures géométriques elles mêmes, je pourrois donner ici des preuves tres intéreſſantes de leur exactitude, par le tableau des ouvrages étonnans qu'on entreprend d'après elles; mais je craindrois d'être trop long. Et d'ailleurs nous pouvons nous en rapporter aux mineurs ſur l'exactitude de ces meſures; ils y ont un plus grand interêt que nous. Je conclurai donc ce memoire par quelques remarques générales ſur la meſure barométrique.

So long as in this meaſurement we ſhall conſider as given only the differences in the weight and heat of the air at the places of obſervation, we ſhall be ſubject to errors; becauſe there are many other cauſes of modification in the air: and all the exactneſs to which we can pretend will be to determine a formula which preſerves a mean among the poſſible variations.

This is what I have propoſed to do in my own formula, and it ſeems to anſwer this end. In the different trials which have been made of it, it has ſometimes given the heights too great, at other times too little, without diſtinction of climate. Thus, for example, the experiments at Spitzbergen by Lord MULGRAVE, and at the Pike of Teneriffe by Mr. DE BORDA, one of the French academicians, gave the heights too great; thoſe of Col.

Tant qu'on n'aura pour données dans cette meſure, que les differences de poids et de chaleur de l'air aux lieux des obſervations, on ſera ſujet à des erreurs; parce qu'il y a bien d'autres cauſes qui modifient l'air: et toute l'exactitude à laquelle on pourra prétendre, ſera de déterminer une formule qui garde le milieu entre les écarts poſſibles.

C'eſt là ce que je me ſuis propoſé dans la mienne, et elle paroit répondre à ce but. Dans les diverſes épreuves qui en ont été faites, elle a donné quelquefois les hauteurs trop grandes, d'autres fois trop petites, ſans diſtinction de climat. Ainſi par exemple; éprouvée au Spitzberg par mylord MULGRAVE, et au Pic de Ténériffe par M. DE BORDA l'un des Académiciens François, elle a donné les hauteurs trop grandes; et dans les obſervations de M. le Col. ROY et de M. le Chev.

Col. ROY and Sir GEORGE SHUCKBURGH*, made in mean latitudes, and partly in the places where I myself had observed, gave them too little.

These differences, do not seem to depend on the climate, and indeed I have frequently observed them to happen in the same places. Thus, for example, my observation on the Glaciere de Buet, cited by Sir GEORGE SHUCKBURGH, gives the height of that mountain a little less than the geometrical measure; but Mr. DE SAUSSURE having repeated the barometrical observation, it agreed with that measure by the same formula; and Mr. MARC PICTET, by a third observation, found the height a little too great. In these three observations, the particulars of which I omit, the corresponding point was Geneva, distant about ten or twelve leagues.

At

Chev. SCHUCKBURGH, faites dans des latitudes moyennes, et en partie dans les lieux où j'ai moi même observé, elle les a donné, trop petites.

Ces différences ne paroissent donc pas tenir aux climats; et en effet je les ai observées fréquemment dans les mêmes lieux. Ainsi par exemple, mon observation sur la Glaciere de Buet, citée par M. le Chev. SCHUCKBURGH*, donne la hauteur de cette montagne un peu moindre que la mesure géométrique: mais M. DE SAUSSURE ayant répété l'observation barométrique, se trouva d'accord avec cette mesure, par la même formule; et M. MARC PICTET, dans une troisième observation, trouva la hauteur un peu trop grande. Dans ces trois observations, dont je ne rapporte pas les détails, le point correspondant étoit Géneve, distant de 10 à 12 lieues.

* Phil. Transf. 1777.

A cette

At that diſtance there are doubtleſs ſome cauſes of variations which are irremediable; ſince the formula ſuppoſes that the obſervations are made in the ſame column of air. It is therefore only in the caſes in which that ſuppoſition approaches near the truth that we can hope to perfect the rule. But this can only be by introducing new conditions into it; that is, other modifications of the air, of which we have not as yet taken any account.

In meditating on the cauſes of the diverſity of reſults in experiments, it has always appeared to me, that the differences of the effects of heat on the air, according to the different ſtates of it, was the principal; that is, that the air not being always of the ſame nature, heat, that grand cauſe, whoſe effects we ought principally to determine, does not always act equally. Beſides the particular experiments

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A cette diſtance ſans doute il y a des cauſes d'écarts qui ſont irrémédiables; puiſque la formule ſuppoſe que les obſervations ſont faites dans une même colonne d'air. Ce n'eſt donc que pour les cas où cette ſuppoſition approche de la verité, qu'on peut eſpérer de perfectionner la régle. Mais ce ne ſera qu'en y introduiſant de nouvelles conditions; c'eſt à dire d'autres modifications de l'air dont on n'a pas tenu compte juſqu' ici.

En étudiant les cauſes de la diverſité des réſultats dans les expériences, il m'a toujours paru, que la différence des effets de la chaleur ſur l'air, ſuivant qu'il eſt lui même conſtitué, étoit la principale: c'eſt à dire, que l'air n'étant pas toûjours de même nature, la chaleur, cette grande cauſe dont il faut principalement déterminer les effets, n'y agit pas toujour également. Outre les expériences

periments which prove it, we can attribute to these differences only those of the results of the researches of some philosophers concerning the dilatations of the air by heat, applied to various physical uses.

In a paper lately read at the Royal Society, on the subject of refractions, I analysed and compared different formulæ of this kind, given by philosophers on whom we can depend. The result of that examination was that, supposing the volume of air = 1000 when the English thermometer of FAHRENHEIT is at 32°, if the heat of the air be increased 22,8 degrees of this thermometer, its volume will be,

According

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particulières, qui le prouvent, on ne peut attribuer qu'à ces différences, celles des réfultats des recherches de divers phyficiens fur les dilatations de l'aïr par la chaleur, appliquées à divers ufages phyfiques.

C'eft à l'occafion des réfractions, que, dans un mémoire lu il y a peu de tems à la Société Royale, j'ai analyfé et comparé différentes formules de cette efpéce, données par des phyficiens, fur qui l'on peut compter. Il eft réfulté de cet examen, qu'en fuppofant le volume de l'air = 1000 lorfque le thermométre Anglois de FAHRENHEIT eft à 32°, fi la chaleur de l'air augmente de 22,8 degrés de ce thermométre, fon volume fera:

Suivant

According to Mr. l'Abbé DE LA CAILLE, 1040
- - - Mr. Profeſſor MAYER, 1046
- - - Mr. BONNE, - - 1047,7
- - - Sir GEORGE SHUCKBURGH, 1050,5
- - - Dr. BRADLEY, - 1054,4

Here then are great differences in this point only, namely, the effect of heat on the denſity of the air; differences which muſt have a viſible effect in all meteorological phenomena. Doubtleſs they proceed, in a great meaſure, from the different degrees of dryneſs of the air, which we can no longer doubt of, ſince the intereſting experiments of Col. ROY with the manometer *. This is the ſame cauſe to which I imputed the greateſt variations in my experiments in open air, and which obliged me to conclude

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Suivant M. l'Abbé DE LA CAILLE, 1040
Suivant M. le Prof. MAYER, 1046
Suivant M. BONNE, 1047,7
Suivant M. le Chev. SHUCKBURGH, 1050,5
Suivant M. le Dr. BRADLEY, 1054,4

Voilà donc de bien grandes différences ſur ce ſeul point, l'effet de la chaleur ſur la denſité de l'air; différences qui doivent agir dans tous les phénoménes météorologiques. Elles procédent ſans doute en plus grande partie des différens degrés de ſéchereſſe de l'air; on ne peut plus en douter depuis les intéreſſantes expériences de M. le Col. ROY avec le manomètre *. C'eſt cette même cauſe que j'avois ſoupçonnée de produire le plus d'ecarts dans mes expériences en plein air, et qui m'obligea à conclure ma correction pour les effets de la chaleur ſur

* Phil. Tranſ. 1778.

conclude my correction for the effects of heat upon the air from a mean among my numerous obſervations. Now this mean, reduced to the ſame term of compariſon as above, is 1047, which is alſo a mean among thoſe different reſults.

Theſe differences therefore ſhew us, that atmoſpherical air is not a ſubſtance that is homogeneous and conſtantly the ſame; and that next to the care of making as many obſervations as poſſible in the maſs of air itſelf which it concerns us to know, the only means of determining more exactly its various influences is to multiply our meteorological inſtruments.

Barometrical

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l'air, par un milieu entre mes nombreuſes obſervations. Or ce milieu, reduit au même terme de comparaiſon ci deſſus, eſt 1047, qui ſe trouve être auſſi le milieu entre ces divers réſultats.

Ces différences nous montrent donc; que l'air atmoſphérique n'eſt pas une ſubſtance homogéne et conſtamment la même; et qu' après le ſoin de faire autant qu'il eſt poſſible, les obſervations dans la maſſe même de l'air qu'il nous importe de connoître, le ſeul moyen de déterminer plus exactement ſes diverſes influences, eſt de multiplier nos inſtrumens de météorologie.

Barometrical Observations in the Deep St. John, July *the 5th*, 1778.

	Time.	Bar. corrected by its therm.	Therm. in the air.
	h ′	Inches.	
At the ent. of the mine,	4 50 night	26.6224	– 5
	2 5 night	26.6276	– 7½
The mean,	-	26.625	– 6¼
At the George Gallery,	5 50 night	27.4167	–17
	8 15 night	27.4271	–17
The mean,	-	27.4219	–17
At the level of the 11th G.	7 30 night	27.9792	– 8

Calculation

Observations barométriques au Profond St. Jean, *le 5e* Juillet, 1778.

		Bar. corrigé par son therm.	Therm. dans l'air.
	h ′	P. L.	
A l'entrée de la mine,	4 50 ſoir.	26.7 13/16	– 5
	9 5 ſoir.	26.7 7/16	– 7⅛
	Terme moyen,	26.7 5/16	– 6¼
A la galerie George,	5 50 ſoir	27.5	–17
	8 15 ſoir	27.5 5/16	–17
	Terme moyen,	27.5 1/16	–17
Au niveau de l'onzième gal.	7 30 ſoir	27.11 13/16	– 8

Calcul

Calculation of the Depth of the George Gallery.

Barometer at the gallery,	27.4219 = 5265 (192ds of an inch) Log.	37213984
At the entr. of the mine,	26.6250 = 5112	37085908
	Difference	128076
Heat of the air at the gallery,	− 17	
At the entrance of the mine,	− 6¼	
	− 23¼ = $\frac{23\frac{1}{4}}{1000}$ to deduct	2977
	1000dths of a French fathom,	125099
	$\frac{1}{61}$ to add, − −	2051
	Lachters	127.150

Calculation

Calcul de la profondeur de la Galerie George.

Bar. à la Gal. −	27.5.1 = 5265 (16^{mes} de lig.) Log.	37213984
A l'entrée de la mine,	26.7.8 = 5112	37085908
	Difference	128076
Chaleur de l'air à la Gal −	− 17	
A la entrée de la mine, − −	− 6¼	
	− 23¼ = $\frac{23\frac{1}{4}}{1000}$ à deduire	2977
	1000^{me} de toise de France, −	125099
	$\frac{1}{61}$ à ajouter, − −	2051
	Lachters	127,150

Calculation of the Depth of the 11*th gallery.*

Barometer at the gallery,	27.9792 = 5372	Log.	37301360
At the entrance of the mines,	26.6250 = 5112		37085908
	Difference		215452
Heat of the air at the gallery,	– – 8		
At the entrance of the mine,	– – 6¾		
	$-14\frac{3}{4} = \frac{14\frac{3}{4}}{1000}$ to deduct		3070
	1000dths of a French fathom,		212382
	$\frac{1}{61}$ to add, – –		3482
	Lachters		215.864

Calcul de la profondeur de l'onziéme galerie.

Bar. à la galerie, –	27.11$\frac{3}{16}$ = 5372	Log.	37301360
A l'entrée de la mine –	26. 7$\frac{5}{16}$ = 5112		37085908
	Difference		215452
Chaleur de l'air à la gal. –	– 8		
A l'entrée de la mine, –	– 6¾		
	$-14\frac{3}{4} = \frac{14\frac{3}{4}}{1000}$ à deduire		3070
	1000^{me} de toise de France,		212382
	$\frac{1}{61}$ à ajouter, – –		3482
	Lachters		215,864

XXXII. *On the Preceſſion of the Equinoxes produced by the Sun's Attraction. By the Rev. Mr.* Iſaac Milner, *M. A. and Fellow of* Queen's College, Cambridge; *communicated by the Rev. Dr.* Shepherd, *F. R. S.*

Read June 24, 1779.

IF the actions of the Sun and Moon upon the different parts of the earth were equal; or if the earth itſelf were perfectly ſpherical, and of an uniform denſity from the center to the ſurface; in either caſe the attractions of thoſe remote bodies would have no effect on the poſition of the terreſtrial equator, and the equinoctial points would conſtantly be the ſame in the heavens. But it was impoſſible to give the earth a rotatory motion round an axis without giving at the ſame time a centrifugal force to its parts. This force is greateſt at the equator, and is in a contrary direction to that of gravity; on either ſide of the equator the force is leſs; and, moreover, only part of its effects is oppoſed to that of gravity. It is uſual in determining the figure of the earth to conſider the whole maſs as in a ſtate of fluidity, and the different columns as ſuſtaining one another at the center. If the earth

earth be conſidered as a hard body, firmly cohering in its parts by ſome other force beſides that of gravity, it does not ſeem neceſſary that the different columns ſhould be ſuppoſed to ſuſtain each other at the center, though in both caſes the direction of gravity muſt at every point of the ſurface be perpendicular to the tangent of the figure. But we know, that there is a conſiderable quantity of water upon the ſurface of the earth; and, therefore, if the equatorial regions were not higher than the polar, they certainly would be overflowed by the Ocean, which is contrary to experience; and for this reaſon the proportion of the diameters of the earth, determined upon the falſe ſuppoſition of an entire fluidity, cannot differ much from the truth.

§ 2. But the preceſſion of the equinoxes, which depends upon the unequal actions of the Sun and Moon on the protuberant parts of the earth at the equator, will not be the ſame in theſe different hypotheſes; at leaſt we can never be certain that it will be ſo until we have computed their effects, and the computation itſelf muſt proceed on different principles. Suppoſe the earth to be fluid under the form of an oblate ſpheroid; or, what is more ſimple, ſuppoſe the region of the equator to be ſurrounded with a ring of fluid matter, and the unequal action of the Sun will diſturb the figure of the ring, and communicate

communicate a motion to its parts. Suppoſe we knew the preciſe diſturbing force of the Sun upon any one particle of this ring according to its ſituation; in that caſe we could eaſily find the velocity which would be communicated to ſuch a particle in any given time; but the mutual actions of the fluid particles upon each other could never be exactly eſtimated, much leſs their effects in endeavouring to turn the whole earth round its center. However, it is eaſy to ſee, that in the caſe of a hard ring of matter cohering cloſe with the ſurface of the earth at the equator, both the law by which the particles act on each other, and on the whole maſs of the earth, will be widely different from the caſe of fluidity, and the effects much greater in altering the poſition of the axis of rotation.

To explain this by an eaſy example (fig. 1,) let A, B, and C, repreſent three ſmall bodies in the ſame horizontal line AE. Suppoſe A to deſcend by any accelerating force as gravity; B to deſcend by the ſame force, a leſs or a greater; and C not to be acted upon at all: in every one of theſe caſes the bodies A and B will deſcend with their reſpective velocities, and the body C will preſerve its ſituation. If A and B are ſmall particles of fluid of any form, and C a hard one, and if the particle A be placed

in contact with B, have its center of gravity a little above the center of gravity of B, and is acted upon by the greater accelerating force; in this case we may conceive how the action of A may disturb the motion of B, and in the same way how the hard particle C may receive a small motion from the actions of A and B. Then this motion must be extremely little compared with the whole motions of A or B, and still a great deal less if C be strongly connected with a string of hard particles along the line CE, so that C cannot be moved without the whole line CE turning round the immoveable center E. Now if A, B, and C, be supposed hard particles firmly connected to the lever AE, then it is plain that the velocity of C, whatever it is, must be in proportion to that of A and B as their respective distances from E the center of motion, and this, whatever the impulsive forces are with which A and B are urged in their respective directions.

The body C being still supposed void of gravity, let the bodies A and B be urged by forces perpendicular to AE in any small equal times through the unequal spaces *s* and *ś*, and let the magnitudes of the bodies be represented by A, B, and C respectively. Then the space through which A is actually urged in that time will easily appear from mechanics to be represented by

$\frac{A \times AE \times s + B \times BE \times s \times AE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$, and the space described by C is

$\frac{A \times AE \times s + B \times BE \times s \times CE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$. See article the 13th.

§ 3. The preceding article being well understood, whatever doubts may remain concerning the motion of a ring of matter considered as detached from the earth, we may be certain that the motion of the nodes of the equator can never be the same, whether we suppose the ring at the equator to be fluid and to rest upon the surface of the earth, partaking of the diurnal motion, or whether we suppose it hard and compact, and by its cohesion communicating a proportional degree of motion to the different parts of the earth. In fact, the problem of the precession of the equinoxes, which has hitherto been considered as extremely difficult, and in its solution drawn out by authors to an immeasurable length, requires no principles but the received doctrine of motion, and the application of the lever, which have been made use of in the last article. In that article we supposed the bodies A and B to be impelled by different forces in parallel lines, and we estimated the real space, which either A or C in any small time would describe in consequence of those impulsive forces and their mutual connection by an inflexible lever. Now this is precisely what is re-

quired to be done in the caſe of the Sun's unequal action on the protuberant parts of the equator. The exceſſes or defects of that unequal action are to be conſidered as forces applied to thoſe parts, which would move them according to the different circumſtances through unequal ſpaces proportional to the forces in equal times of action, provided the particles were at liberty to move freely in the directions in which they are urged; and, laſtly, the real ſpace muſt be computed through which a particle moves at ſome known diſtance from the center of the earth in conſequence of theſe various forces. This whole proceſs will not differ from the eaſy example already deſcribed, except in the length of the calculation, and the proper management of the doctrine of fluxions; and it ſeems adviſable in difficult ſubjects always to begin with ſimple inſtances before we proceed to thoſe which are more complex, and to diſtinguiſh the algebraical operations from the principles upon which they are founded.

§ 4. In order to determine how much any particle of the earth is affected by the unequal action of the Sun (fig. 2.), let CADB repreſent the earth, S the Sun at a great diſtance, and CD a plane perpendicular to the line ST joining the centers of the Sun and earth. If SK or ST repreſent the accelerating force of the Sun on a particle at the earth's center, and SL be taken to SK in the duplicate ratio

ratio of ST to SP; SL will represent the attraction on any particle P, and by the resolution of motion T*m* or PL will represent the perturbing force of the Sun on the same particle. By the construction $SL : SK :: SK^2 : SP^2$, and by division $KL : SK :: SK^2 - SP^2 : SP^2 :: \overline{SK + SP} \times PK : SP^2$ and PL or T*m* is nearly equal to 3PK, and as 3PK is to SK or ST, so is the space described by P in any small time in the direction PK, to the space described in the same time by the center of the earth in consequence of the Sun's attraction. This last space is equal to $\frac{\dot{x}^2}{2ST}$ where $\dot{x}$ represents the arc described by the earth's center during any small motion in its orbit, and the former is equal to $\frac{3PK \times \dot{x}^2}{2ST^2}$. This is the space which would be described by P in the direction PK if the particle was at liberty to move freely. Let us at present suppose that no other particle is disturbed by the Sun's attraction except this one, and then proceed to enquire into the effects of this disturbance when P by its cohesion communicates a motion to the different parts of the earth, which is farther constrained to turn round an axis T, the common intersection of the plane CD and the terrestrial equator. From the laws by which motion is communicated, and the property of the lever, it easily appears, as in the second article, that the space through which any particle of the

earth's equator is impelled at the greateſt diſtance from the axis T, is to $\frac{3PKc^3}{2sT^2}$, the ſpace which would be deſcribed in the ſame time by any particle at liberty, the magnitude of which is repreſented by P, as $P \times KT \times$ the radius of the equator to the ſum of all the particles of the earth multiplied into the ſquares of their reſpective diſtances from the ſaid axis.

To compute this ſum in the eaſieſt way, and by an approximation, which is quite ſufficient when the polar and equatorial diameters differ little from one another; let DPE (fig. 3.) be a ſphere whoſe radius is unity, divided into an infinite number of thin cylindrical ſurfaces, whoſe baſes are the circles NAQ; it is obvious, that all the particles in any one of theſe ſurfaces are at the ſame diſtance CA$=x$ from the axis of motion perpendicular to the plane of the circle NAQ. Call AP, y, and A, the area of the circle DPE and the fluent of $4Ax^3\dot{x}y$, or of $-4Ax^3y^2\dot{y}$, becauſe $x\dot{x} = -y\dot{y}$ gives the ſum of all the particles in the ſphere multiplied into the ſquares of the reſpective diſtances from the axis. This fluent corrected is equal to $\frac{8A}{15}$, and muſt now be diminiſhed in the ratio of 1 to $1-2p$, if we ſuppoſe the earth to be an oblate ſpheroid whoſe equatorial diameter is to the polar as 1 to $1-p$;

and, lastly, the space described by a particle at the greatest distance from the axis is equal to $\frac{45P \times PK \times KT \times \dot{z}^3}{16A \times ST^2 \times \overline{1-2p}}$.

§ 5. In fig. 4. let PIA*p*DK represent the earth orthographically projected on the plane of the solstitial colure, P, *p*, the poles, IK a lesser circle parallel to the equator, and P*ape* a sphere described with the polar radius PT: then, since the particles without the globe only are concerned in changing the position of the axis of rotation, let L represent such a particle situated in the circumference of the circle IK, and by the preceding article its effect will be $\frac{45L \times LM \times MT \times \dot{z}^3}{16A \times ST^2 \times \overline{1-2p}}$, and by the same way of reasoning, when two equal particles L, *l*, are supposed to be disturbed by the Sun's attraction, the space described by that point of the equator, which is at the greatest distance from the axis of rotation or the common intersection of the plane CD and the equator A, will be equal to $\frac{45\dot{z} \times \overline{L \times LM \times MT + l \times lm \times mT}}{16ST^2 \times A \times \overline{1-2p}}$, and the same argument holds for every other particle without the sphere.

The sum of all the $L \times LM \times MT + \&c.$ must now be found; and for this purpose Sir ISAAC NEWTON's construction is, perhaps, as convenient as any that has hitherto appeared. In the same figure N*n* is parallel, and *xy* perpendicular, to CD; take L*x*=*xl*, and let *m*, *n*, represent

ſent the ſine and coſine of the angle CTP to the radius unity. It is eaſy to prove in his way that $L \times LM \times MT + l \times lm \times mT$ is equal to $2L \times m \times n \times \overline{Lx^2 - Tx^2}$, and the fluent of Lx^2 multiplied into the fluxion of the circular arc Lx is eaſily found in the following manner, without having recourſe to tables of fluents, or the methods of continuation.

From a known analogy the fluxion of the arc Lx is to the fluxion of its verſed ſine as the radius $1x$ of the ſame circle to Lx the right ſine. Lx multiplied into the fluxion of the verſed ſine is the fluxion of the area of the ſemi-circle Ll, and calling $1x, y$, the fluent of Lx^2 multiplied into the fluxion of the arc Lx is evidently equal to $\frac{y^3 A}{2}$, where A ſtill repreſents the area of a circle whoſe radius is unity: Ay is equal to the ſemi-circumference IK and $Ay \times Tx^2$ is equal to the fluent of Tx^2 multiplied into the ſame fluxion, and calling Tx, v, and ſubſtituting for $1i$ its equal py, the ſum of all the $L \times LM \times MT$ + &c. in the annulus $1i$ is equal to $mnpA \times \overline{y^4 - 2y^2v^2}$. This laſt quantity multiplied into the fluxion of v, and the fluent taken by the common method when v is equal to TP or unity nearly, comes out $\frac{4pAmn}{15}$ and twice this quantity gives the ſum of all the $L \times LM \times MT$, without the whole ſphere Pap, and therefore the ſpace deſcribed by a particle of the equa-

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tor in the circle of the Sun's declination while the center of the earth is carried through the ſpace $\dot{z}$ of its orbit is equal to $\frac{3pmn\dot{z}^2}{2sT^2 \times 1-2p}$, and may be ſuppoſed to be equal to $\frac{3pm\dot{z}^2}{2sT^2}$, the alteration produced by the correction in art. 4. on account of the ſpheroidical figure of the earth being too inconſiderable to affect the concluſion.

§ 6. We are to obſerve, that the ſpace $\frac{3pmn\dot{z}^2}{2sT^2}$ deſcribed by that point of the equator, which is the interſection of the circle of the Sun's declination, is generated by the perpetual attraction of the Sun. This attraction may be reckoned conſtant during the very ſmall time of the earth's deſcribing $\dot{z}$ in its annual motion; and, therefore, the ſaid point of the equator, at the end of that time, will have acquired a velocity which would carry it through $\frac{3pmn\dot{z}^2}{sT^2}$ in the ſame time.

§ 7. Let T repreſent the time of the earth's revolution in its orbit, t the time of its rotation round its axis, and ſuppoſe $\dot{w}$ to be a ſmall arc ſimilar to $\dot{z}$ in a circle whoſe radius is unity. In figure 5. let AQ be the equator, and take Ab equal to $\frac{\dot{w}T}{t}$, and bt perpendicular to Ab equal to $3pmn\dot{w}^2$, and Ab, bt, will repreſent the directions and quantities of the two different motions of the point A, and conſequently At will be the direction of the

new equator, and as Ab or At is to bt, ſo is the radius unity to the ſine of the angle tAb, and if AQ or AG be taken equal to a quadrant, GQ the meaſure of the angle GAQ is equal to $\frac{3\dot{p}mn\dot{w}t}{T}$.

§ 8. Suppoſe s the Sun's place in the ecliptic NS, N the equinoctial point, NA the Sun's right aſcenſion, and ro a perpendicular on AN: then ro is to GQ as the ſine of AN to the radius, and rN to ro as the radius to the ſine of the angle at N the inclination of the ecliptic to the equator, and, *ex æquo perturbatè*, rN to GQ as the ſine of AN to the ſine of N and rN the ſmall preceſſion of the equinoxes is equal to $3\dot{p}mn\frac{\dot{w}\times t}{T}\times\frac{\text{ſin. NA}}{\text{ſin. N}}$.

§ 9. In the ſpherical triangle ASN, the ſine of AN = cotang. N × tang. AS $=\frac{m\times\text{cof. N}}{n\times\text{ſin. N}}$, and farther the ſine of SN $=\frac{m}{\text{ſin. N}}$ and rN is equal to $\frac{3\dot{p}t\times\overline{\text{ſin. }w}^{2}\times\dot{w}\times\text{cof. N}}{T}$, whoſe fluent or $\frac{3\dot{p}t}{4T}\times\overline{2w-\text{ſin. }2w}\times\text{cof. N}$ gives the preceſſion of the equinoxes during the Sun's motion through the arc NS of the ecliptic: when NS is equal to a circle, then the whole fluent becomes equal to $\frac{3\dot{p}tA}{T}\times$ cof. N, and as 2T is to $3\dot{p}t\times$cof. N, ſo is $60\times60\times360$ to $21''+6'''$ the annual preceſſion of the equinoxes in ſeconds produced by the Sun's attraction.

§ 10.

§ 10. We might now proceed in a ſimilar way to inveſtigate the effects of the Moon's diſturbing force, the rotation of the earth's axis, and the equation of the preceſſion; but ſince theſe propoſitions are purely mathematical, and the computations have already been gone through by other authors, it will be needleſs to repeat them here.

§ 11. NEWTON was the firſt who attempted to explain the preceſſion of the equinoxes from its cauſes. Since his time various other ſolutions have been given us by the moſt celebrated mathematicians; and it deſerves to be noticed, that, in a caſe where there can be little doubt that he was miſtaken, other authors have found it difficult to agree among themſelves in differing from him. M. D'ALEMBERT, in the year 1749, printed a treatiſe expreſsly on the ſubject, and has ſince ſaid [a], that himſelf is acknowledged to be the firſt who determined rightly the method of ſolving ſuch problems. EULER, DE LA GRANGE, FRISIUS, SILVABELLE, WALMESLEY, SIMPSON, EMERSON, have each conſidered the ſubject, and perhaps the importance of the enquiry would juſtify a minute examination into the cauſe of the agreement or diſagree-

(a) D'ailleurs, des géometres, vraiment capables d'apprécier mon travail, ont abondamment ſuppléé à tout autre temoignage, en declarant que j'ai ouvert le premier la route pour reſoudre ce genre de queſtions. See Opuſc. Math. vol. V. ſur la Préceſſion des Equinoxes.

ment of their ſeveral methods; but I am deterred from entering into ſuch a diſcuſſion by the length of time which it would require; eſpecially as I think thoſe who have read the authors mentioned will eaſily conceive the ſubſtance of what I ſhould have to obſerve, and to thoſe who have not read them I ſhould hardly be able to ſay any thing intelligible.

§ 12. The above ſolution, if it had no other advantages, is, I apprehend, much more conciſe than any that has hitherto been given. Abſtracted from what is ſaid by way of illuſtration, articles 4th to 9th contain all the calculation requiſite, and as I have ſtudiouſly avoided the ambiguous uſe of the terms *force*, *vis*, *efficacia*, *momentum*, &c. as well as every doubtful repreſentation of times, ſpaces, and velocities, which are often ſubſtituted by authors in equations, I believe the whole proceſs will appear eaſy, and the evidence upon which the concluſion reſts be exactly aſcertained.

§ 13. The principles deſcribed in articles 2. and 4. depend upon the third law of motion, and the property of the lever, and are demonſtrated in the following manner. Every thing remaining the ſame as in art. 2. (fig. 6.) let AV and BR, perpendicular to the right line or axis AE, repreſent the forces and directions with which thoſe bodies are reſpectively urged, when at liberty to move

freely in those directions; and let A v, B r, C c, represent the accelerative forces of the respective bodies, as altered by their mutual actions upon each other: then, because C × C c is the moving force gained by C, and A × vV + B × rR the moving force lost by A and B, regard being had to the lengths of the different levers AE, BE, we shall have A × vV × AE + B × rR × BE equal to C × C c × CE, that is, A × AE × AV − A v + B × BE × BR − B r equal to C × C c × CE, and by transposition A × AE × AV + B × BE × BR equal to C × CE × C c + A × AE × A r + B × BE × B r. Let s, s', represent, as in art. 2. the spaces which would be described by the bodies A and B at liberty in any very small portion of time, and let x be the space which A actually describes in that time when connected with B and C by the lever AE. The quantities $\frac{x \times BE}{AE}$, $\frac{x \times CE}{AE}$, will then be the spaces described by B and C respectively; and, lastly, because the spaces described in given times are as the accelerating forces, the above equation gives x equal to $\frac{A \times AE \times s + B \times BE \times s' \times AE}{A \times AE^2 + B \times BE^2 + C \times CE^2}$.

The same method extends itself easily to more difficult cases, and by its assistance several very important theorems are briefly demonstrated.

§ 14. The reasoning made use of in art. 6. will appear very evident to any one moderately versed in the

elements of mechanics and the doctrine of moving forces; and therefore I must believe that it is by mistake that one author of note entirely omits so necessary a step which affects the conclusion by just one half. When a body moves with any velocity in the direction AM (fig. 7.) which would carry it through the space AD in a small particle of time, and any force which may be reckoned constant for that time urges the body through the space DC perpendicular to AM, the body at the end of that time will arrive at the point C; but joining AC we are not to suppose, that, if that force ceased to act, the body would proceed in the direction ACL: for take C*m* equal and parallel to AD, and C*d* in CD produced equal to 2CD, and the direction of C at that point will be C*l*, the diagonal of the parallelogram C*dlm*.

Thus when a body revolves in any curve by a centripetal force (fig. 8.) we may, with Sir ISAAC NEWTON, suppose the curve to be composed of an indefinite number of right lines, and the body to move either in the chords or the tangents of the curve; but then we are to take care that we make not suppositions inconsistent with each other. Let the curve be a circle, and AD a tangent at the point A the direction of the body's motion when it arrives at that point, and let DC, parallel to the diameter AL be the effect of the centripetal force: then, if we

suppose the body to move along the chord AC, and say, that the angle CAD measures the deflection of the path in the time of the body's moving through the arc or chord AC, we shall mistake by one half of the true quantity; for draw the tangent at c, and since A*d* is equal to *d*c from the property of the circle, the angle c*d*D of deviation is equal to twice the angle CA*d*. The practice of NEWTON in a similar case, where he is investigating the horary motion of the lunar nodes in a circular orbit, is entirely consistent with this. See the Principia, lib. III. prop. 30.

§ 15. M. D'ALEMBERT has lately charged SIMPSON's account of the precession of the equinoxes with some mistakes of this nature in his second lemma; but, in justice to SIMPSON, I must say, that, whatever other defects there may be in his paper, I am convinced, after the most diligent attention, that those alluded to are without foundation.

§ 16. Sir ISAAC NEWTON first observed, that an homogeneous globe could not possibly retain many distinct motions, without compounding them all into one, and revolving with a simple and uniform motion about an invariable axis. When two forces impress upon a globe two distinct circular motions [b], he briefly concludes in

(*b*) See Principia, lib. I. prop. LXVI. coroll. 22.

his

his way, from the laws of motion, that it is the ſame thing as if thoſe two forces were at once impreſſed in the common interſection of the equators of thoſe motions, and upon this principle we ſuppoſed A*t* in art. 7. to be the direction of the new equator. In order to remove any doubts that might ariſe about the juſtneſs of this mode of compounding motion, FRISIUS has given a geometrical demonſtration of the principle: but the thing may be ſhewn much more eaſily in the following manner. Suppoſe (fig. 9.) RB, AB, to be two axes about which every point in the plane ABPR tends to move with velocities as the reſpective diſtances from the axes; let PQ perpendicular to AB be to PR perpendicular to RB as the angular velocity of P about RB to the angular velocity of the ſame point about AB, and let the velocities be in contrary directions: then, I ſay, every point in the plane will move with a velocity proportional to its diſtance from the axis PB. Firſt, it is evident, that any point C in the axis RB will move round PB with a velocity proportional to its diſtance C*m*: for the point C lying in the axis RB has no velocity round RB, and CM is proportional to CN. Draw P*c* parallel to AB, and any point D in that line will move with a velocity proportional to DV, which is perpendicular to PB, for the following reaſon.

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The velocity of D is equal to the difference of its two velocities round the reſpective axes RB, AB, or the difference of D's velocity round RB, and P's velocity round AB, ſince all the points in PC move with the ſame velocity round AB. Draw DT parallel to CR, and this difference will be proportional to PT, becauſe the velocities of P round RB, AB, are ſuppoſed equal to each other, and PT is proportional to DV, and every point in the plane moves round PB with a velocity proportional to its diſtance; and the ſame thing may be ſhewn when any point is taken without the plane ABCPR.

§ 17. Becauſe any point C in the axis RB moves with the ſame velocity round PB as it does round AB, the angular velocities round the two axes AB, PB, will be to each other inverſely as their reſpective diſtances CN, C*m*; and becauſe CN : C*m* :: PB : PC and PR : PQ :: PC : CB, it follows, *that* PB *the diagonal of the parallelogram* PGBC *will repreſent the angular velocity of the revolving plane, when* BG, BC, *are taken to each other as the angular velocities of the ſame plane round thoſe reſpective axes.*

§ 18. From this it clearly follows, that the reaſon given by SIMPSON (*e*), in his miſcellaneous tracts, of the difference between his own ſolution and that of NEWTON in the Principia, cannot poſſibly be the true one. "It

(*e*) Pages 44. and 45.

"appears

"appears further," says he, "by perusing his thirty-ninth "proposition, that he there assumes it as a principle, that "if a ring encompassing the earth at its equator, but de- "tached therefrom, was to tend or begin to move about "its diameter with the same accelerative force or angular "celerity as that whereby the earth itself tends to move "about the same diameter through the action of the "Sun, that then the motion of the nodes of the ring and "of the equator would be exactly the same."

The principle is certainly implied in NEWTON's proof, and is capable of the most rigid demonstration, art. 16, 17.

§ 19. It will be asked then, where is the fault of NEWTON's reasoning? How comes his conclusion to be too little by above one half? It is acknowledged on all hands that there is an error in his third lemma; but then the correction of that error makes only a very small alteration in the result.

It is impossible for any one to form a complete judgement of his method without going through the whole of his calculations, which pre-supposes that the mean motion of the lunar nodes is computed. This motion may be concisely determined and exactly enough for the purpose from prop. 30. of the Principia, and from thence is inferred the motion of the nodes of a satellite revolving

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in the plane of the equator at the ſurface of the earth with a velocity equal to that of the earth round its axis. We are then to ſuppoſe, that the *mean* motion of the nodes of a ſatellite, revolving with ſuch a velocity, is the ſame with the motion of the nodes of a ring of rigid matter ſurrounding the earth at its equator, and revolving with the ſame velocity. This laſt hypotheſis is admitted by SIMPSON, who thinks that his own ſecond lemma contains a full demonſtration of the point. For my own part, I believe with FRISIUS, that we are to look here for the material error in NEWTON's ſolution of the problem. It is evident, that the *true* motion of the nodes of the ſatellite, and the ring of matter, are not the ſame; and it is by no means obvious, that their mean motions are ſo. The mean motion of the nodes of a ring of hard matter cohering together is very eaſily computed by the method in art. 4th to the 9th, and turns out nearly double the mean motion of a Moon revolving at the ſurface of the earth with the ſame velocity.

It is a very intereſting enquiry to find out the real cauſe of the miſtake in the Principia, lib. III. prop. 39.; and therefore at a future opportunity I may, perhaps, conſider this particular part of the ſubject more attentively. I have long been ſatisfied with the account already given, and

ſhould probably have remained ſo, if M. D'ALEMBERT [d], in his Opuſc. vol. V. had not perſiſted to affirm, that the mean motion of the nodes of the ring of matter and of the ſatellite were the ſame.

This opinion of ſo celebrated a mathematician raiſes ſcruples in one's mind; and ſhews, that when we venture to differ from Sir ISAAC NEWTON in theſe matters, it is with the utmoſt difficulty that we can arrive at certainty.

(d) Il n'y a de parité que dans le mouvement *moyen* de ces deux anneaux, ou de l'anneau ſolide et de la lune; les mouvemens *inſtantanés* ſont très differens de part et d'autre; ainſi la comparaiſon du mouvement de l'anneau avec celui de la lune, ſerviroit tout au plus à trouver le mouvement *moyen* de l'anneau, ou de la preceſſion des équinoxes, mais nullement à déterminer la nutation de l'axe et l'équation de la preceſſion.

Fig. 1.

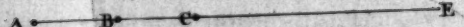


Fig. 2.

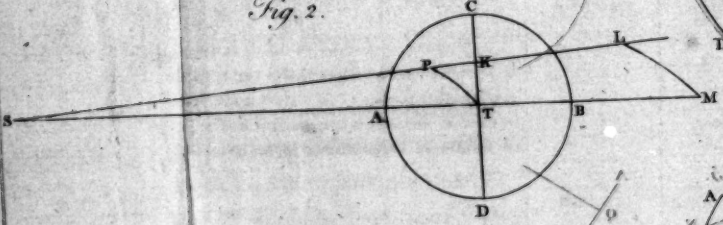


Fig. 3.

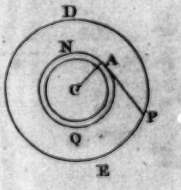
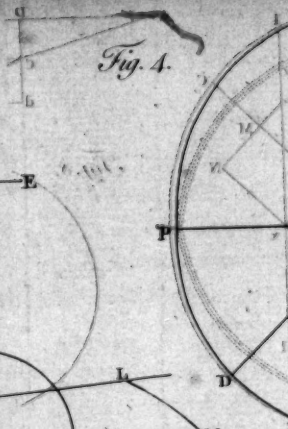
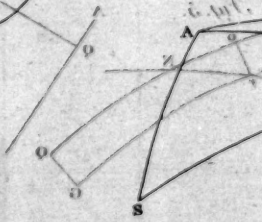
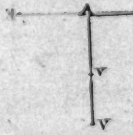
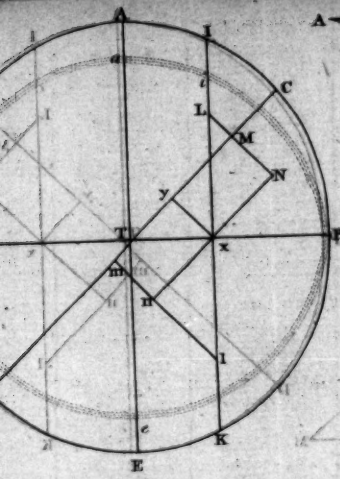


Fig. 4.









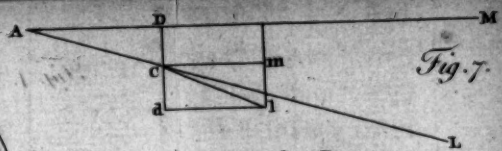


Fig. 7.

Fig. 8.

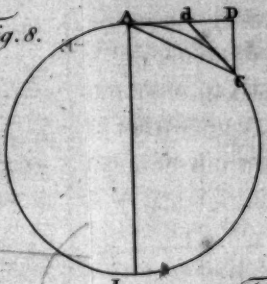


Fig. 9.

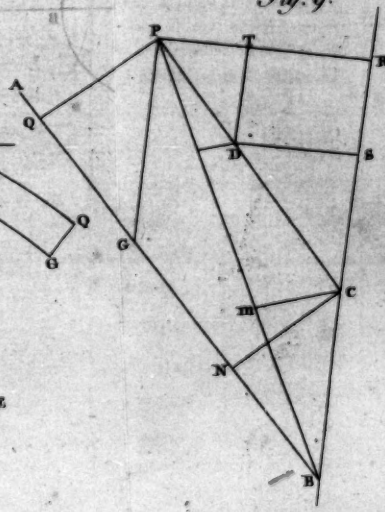


Fig. 5.

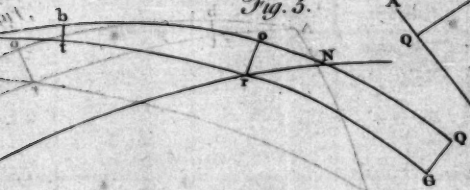
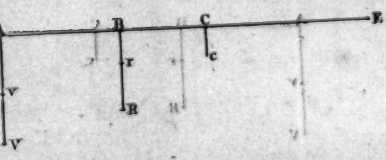


Fig. 6.



XXXIII. *An Examination of various Ores in the Muſeum of Dr.* William Hunter. *By* George Fordyce, *M. D. F. R. S. and Mr.* Staneſby Alchorne.

Read May 20, 1779.

MINERS and aſſay-maſters being generally employed in finding out metals for profit, without inquiring into the ſtate or combination they are in, have made uſe of proceſſes adapted, in many caſes, to inveſtigate the quantity of metal, gueſſing often at its ſtate or combination; and ſpecimens being procured from them for cabinets, an erroneous account has not only been given of their contents, but theſe errors have alſo crept into books of natural hiſtory: for this reaſon Dr. FORDYCE determined to aſſay many of the doubtful ſpecimens, by proceſſes better adapted to find the ſtate or combination of the metal than thoſe commonly in uſe; and procured the aſſiſtance of his friend Mr. ALCHORNE. The reſult of ſome of their inquiries they now lay before the Society.

The first ore we mean to consider is called gold pyrites. Pyrites is an ore of iron, containing sometimes copper, sometimes arsenic, sometimes other metals; generally, if not always, combined with sulphur.

This ore, from its yellow colour, has at first sight been often taken for an ore of gold. It is the most common of all ores, and on examination is very seldom found to contain gold. Specimens, however, have been found from which gold has been procured; and, particularly, there have been found in Fatchobuigna near Zalatna, in Transylvania, masses which contain a large proportion of this metal. Sometimes the gold is in its metallic form, and visible to the naked eye; sometimes it is not: and in these cases the ore has been thought to contain the gold united first with iron, and that compound united with sulphur.

Dr. FORDYCE observes on this proposition, that it has not as yet been proved, that a compound metal can be combined with sulphur. If two metals are soluble in sulphur, and each be separately combined with it, the two compounds may be diffused through one another, as is the case with the compounds of sulphur and iron, and sulphur and copper; which may be diffused through one another: but if we have a compound of two metals, of which one is soluble in sulphur, and the other is not, if

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we apply ſulphur to this compound, it will diſſolve the one, and leave the other; as, if gold and ſilver be combined, if we reduce the maſs to fine particles, and mix them with ſulphur, and throw them into a crucible, heated white hot, and afterwards melt the whole maſs, the ſilver will combine with the ſulphur, and the gold will fall to the bottom of the veſſel. This being the caſe, we doubted whether this ore contains the gold in its metallic form, only mechanically mixed with the pyrites; or combined with the ſulphur by means of the iron: and therefore ſubjected a ſpecimen to examination, in which we could not diſcover, even by the help of a microſcope, any particle of native gold.

EXP. 1. We powdered one hundred grains of this ore, and boiled it in nitrous acid diluted with water; a ſolution took place with effervеſcence. Having digeſted them together, till the whole ſoluble part was taken up, and poured off the ſolution, and made a precipitation by fixed vegetable alkali, the precipitate appeared to be iron. Having waſhed the remaining part with water, and expoſed it in a glaſs matraſs in ſand, to nearly a red heat, a very ſmall portion of ſulphur ſublimed; the remainder was quartzoſe ſand, with particles of gold, which were ſimilar in figure, though ſmall, to the particles of gold found native in veins mixed with various matrixes, and

not at all like particles which had been combined with a *menſtruum*, which ought either to have appeared in a powder, whoſe particles were hardly viſible from their ſmallneſs; or in cryſtals ſimilar to one another.

EXP. 11. If any metal be combined with ſulphur, mercury will not precipitate the ſulphur from it; we therefore took 140 grains of the ſame ore, and triturated it for ſome hours with about five times its weight of mercury: the powdered ore was waſhed off from the mercury, the remainder put into glaſs veſſels, and evaporated by heat: a maſs of gold was left, but part of it being accidentally loſt, its weight could not be aſcertained; it did not amount to above two or three grains at moſt.

The powder, waſhed off and dried, weighed 134 grains: theſe, mixed with as much litharge, and four times their weight of fixed vegetable alkali, and one fifth of wheat flower, and the whole melted produced a regulus of lead, weighing 80 grains, which, on coppelling with a few grains of ſilver, and parting in *aqua fortis*, left one ſixteenth of a troy grain of gold.

It is to be remarked, that in great works, where gold is ſeparated from pieces of crucibles, ſand, or other matter, by amalgamation, notwithſtanding the proceſs be

frequently repeated, and with great care, ſome ſmall portion of the gold ſtill remains; ſo that the ſmall quantity left in this caſe might eaſily have either eſcaped the mercury, or, have been left unobſerved in the powder combined with ſmall particles of it.

We may therefore conclude, that in this ore the gold was in its native form, and not mineralized.

EXP. III. We examined gold pyrites mixed with quartz, with a deep magnifier, and found evidently native gold interſperſed. One hundred grains of this ore, was powdered; and boiled in nitrous acid diluted with water: a ſolution of iron took place, as in the firſt experiment. The *reſiduum* being expoſed to a red heat, there was no appearance of ſulphur; the gold was found in ſuch large particles, and ſo ſimilar to native gold, that we did not think it worth while to apply mercury.

The gold being diſſolved out by *aqua regia*, a quantity of quartz and arſenical ſalt was left.

EXP. IV. When gold and ſilver are found in their metallic form, it is not uncommon to find them mixed; but it ſeldom occurs, that they are mixed in ſo large proportion as in an ore obtained from Norway, and which was given to Dr. HUNTER, by Mr. FABRICIUS.

The

The ore has the appearance of native ſilver, in ſcaly particles, intermixed with a hard quartz, tinged brown in ſome parts by iron. Twenty-five grains of the maſs, where the metal was in the largeſt proportion, melted with litharge, alkali, and phlogiſtic matter, and afterwards coppelled, yielded a globule apparently ſilver weighing two grains which, being boiled in nitrous acid diluted with water, left full nine ſixteenths of a grain of fine gold. Hence one hundred pounds of metal obtained from this ore, conſiſts of ſeventy-two pounds of ſilver, and twenty-eight pounds of gold.

There is an ore of ſilver which is commonly called vitreous (*minera argenti vitrea*, or *argentum vitreum*).

This ore has always been ſuppoſed to conſiſt of ſulphur and ſilver; becauſe, if we melt ſulphur and ſilver together, they form a maſs which reſembles it, eſpecially in colour and malleability; but, as we could find no experiment in any author which authorized this conjecture, we determined to endeavour to aſcertain it by analyſis, Dr. HUNTER not refuſing to ſubject the ore to an aſſay, although ſcarce and expenſive.

EXP. V. Fifty grains of this ore broken in pieces, for it is too malleable to be powdered, were boiled in nitrous acid diluted with water; the acid diſſolved the ſilver with much difficulty, and left a *reſiduum*. The ſolution being poured

poured off, and the *reſiduum* waſhed and expoſed in a glaſs matraſs to a red heat, there was no ſublimate of ſulphur, or any thing elſe obtained. The *reſiduum*, before it was expoſed to heat, did not appear to contain any ſulphur; being of a blackiſh colour after expoſure to heat, it had a number of yellow particles mixed, which eaſily diſſolved in *aqua regia*. Tin being applied to the ſolution made no precipitation, which it would have done if thoſe particles had been gold; hence we ſuppoſed they were iron.

The ſilver was precipitated from the acid by copper: waſhed and dried it weighed four grains, which, on coppelling with lead, loſt one twenty-fourth.

There were particles of powdered quartz apparently mixed with the yellow particles left after the ſolution.

EXP. VI. Being much diſappointed in not finding any ſulphur in the former experiment, we took three hundred grains of the ſame ore. We freed it as well as poſſible from heterogeneous matter: we mixed it with four times its weight of mild fixed vegetable alkali, put them into an earthen body, to which was affixed a glaſs head, and expoſed them to nearly a white heat in a naked fire for a full hour and an half; but no ſublimation took place, there diſtilling only a few drops of water. We then put the body into a melting furnace, and rendered

the whole fluid; as ſoon as it was melted, it was removed from the fire.

The maſs, on concreting, was found divided into two; a black maſs a-top, and a metallic maſs at bottom. The metal being aſſayed by ſolution in nitrous acid and precipitation with volatile alkali, ſhewed no ſign either of gold or copper; loſt nothing by coppellation with lead; it weighed 213 grains, which were pure ſilver.

The black maſs a-top, or *ſcoriæ*, was boiled repeatedly in water, but did not all diſſolve. The inſoluble part was unfortunately thrown away; but to the ſolution we added muriatic acid: on the addition of the acid, there was a ſtrong ſmell of *hepar ſulphuris*, and a copious precipitate, which, on being examined by a microſcope, appeared to conſiſt of pellucid cryſtals, without the ſmalleſt appearance of ſulphur. This precipitation, being expoſed to heat, did not ſmell in the leaſt like ſulphur; it was not in the leaſt inflammable. Excepting then the ſmell of *hepar ſulphuris*, there does not appear any mark of ſulphur in this ore, and a very ſmall particle of inflammable matter dropping in by accident would give this ſmell.

The foregoing experiments occaſioning ſome doubt of ſulphur's being contained in vitrous ſilver ore, we endeavoured to inveſtigate it by other means; and after ſeveral

experiments,

experiments, we made the following one, which ſeems concluſive.

Half an ounce of ſilver was precipitated from nitrous acid by copper, in ſmall flaky cryſtals as uſual; being waſhed and dried, it was mixed with the ſame weight of ſulphur, and put into a crucible, over which another was placed ſo as to cover it, and the two crucibles were luted together, leaving a ſmall aperture for the eſcape of vapour, but not ſo as to admit air for the inflammation of the ſulphur. Theſe being put into the fire, as ſoon as they were heated red-hot, the ſulphur in part eſcaped through the ſmall hole, and as it paſſed through burnt with a blue flame. The fire was increaſed to a ſufficient degree to melt the maſs within; in the mean time the blue flame diſappeared, which ſhewed that no more ſulphur eſcaped.

After we had applied heat enough, as was ſuppoſed, to melt the maſs, the crucibles were removed from the fire, and being ſeparated when cold, a regular button-like maſs was found in the crucible, of a dark lead colour both without and within, brittle, and ſtreaked on the inſide, bearing to be cut with a knife, exactly like the vitreous ore, which it in every way reſembled. The whole maſs gained twenty grains or one twelfth in weight, which may be conſidered as the true weight of the ſul-

phur in this compound, as ſilver combines with ſulphur in its metallic form, without loſing its inflammable air. Fifty Troy grains of this compound was powdered with difficulty, from its toughneſs, and boiled in nitrous acid diluted with water; it diſſolved with difficulty, leaving a very ſmall quantity of a light blackiſh powder, certainly not nearly a grain in weight.

It appears, therefore, that the ſmall portion of ſulphur contained in the vitreous ore may have been decompoſed by the nitrous acid, and the alkali, or the heat, in the former proceſſes; and we may conclude, that vitreous ſilver ore is a compound of ſilver and ſulphur, and when pure, that it contains between ninety-two and ninety-three grains of ſilver in one hundred.

XXXIV. *On ſome new Methods of ſuſpending Magnetical Needles.* *By* John Ingen Houſz, *Body Phyſician to their Imperial Majeſties, and F. R. S.*

Read June 17, 1779.

THE very great utility which navigation derives from the uſe of compaſſes, has been the principal reaſon, that ſo much labour has been beſtowed by many ingenious men in ſearching the beſt method of ſuſpending magnetical needles, and that ſome Academies of Sciences have propoſed conſiderable premiums to be given to him who ſhould ſucceed the beſt in this important object.

It has been, and is ſtill, a general complaint, that ſuch needles as were by their ſize and figure ſuſceptible of the greateſt magnetical power, and which were executed with the greateſt exactneſs, and perfectly balanced, were, for this very reaſon of their ſuperior accuracy, ſo eaſily put in motion, that the ſlighteſt ſhaking of the floor, occaſioned by the obſervers walking in the room, communicates to them ſuch a great quivering motion as to drag them out of their direction, and to render a conſiderable time

time requiſite before they are again fixed in the true magnetical meridian.

Since the late Dr. KNIGHT has improved ſo greatly the method of communicating a conſiderable magnetical force to ſteel bars, needles have been impregnated with a much greater and more permanent polarity than it was poſſible to give them before, and the conſtruction itſelf of compaſſes has been conſiderably improved. But the ſubject ſeems not yet to be exhauſted.

The degree of magnetical power or polarity to be communicated to ſteel well hardened ſeems to depend in ſome reſpect on the proportion which the ſurface of the piece of ſteel to be impregnated bears to its bulk; ſo that a thin lamella of ſteel, weighing for inſtance ten grains, will acquire more magnetical force than a little lump of ſteel of the ſame weight. Thus a ſmall magnet, compoſed of a great many thin lamina of ſteel preſſed together, may be made ſo ſtrong as to ſupport 150 times its own weight, and even more, which has never yet been done by a compound heavy magnet.

If the too great quivering and horizontal motion, or the too great reſtleſſneſs of a ſtrong magnetical needle, is in ſome way or other counteracted by the methods hitherto adopted, the needle is in ſome danger of ſtopping near the true magnetical meridian, without always

pointing

pointing directly in it; becauſe near this very place the power of direction of the needle may be ſo weak as not to overcome the reſiſtance, which was oppoſed to its free horizontal circular motion.

I thought, in the courſe of laſt year, that a great part of the above mentioned difficulty might be taken away by different methods, ſome of which are the following.

EXP. I. I placed an ordinary magnetical needle, ſupported upon its point, in a china baſon, and poured water into it, ſo as to cover the whole needle. The needle loſt a great deal of its reſtleſſneſs, was influenced by the approach of a magnet at a conſiderable diſtance, and ſeemed to point as well as before to the magnetical meridian, though with a ſlower motion.

EXP. II. I took a ſtrong flat magnetical needle, having in its center a round hole, but no cap. I fixed to this needle as much cork as was neceſſary to make it juſt ſwim upon the water in a baſon. I fixed a ſmooth braſs pin in a vertical poſition, ſo that it paſſed through the hole in the center of the needle, to prevent its ſwimming to the ſides of the baſon. This did tolerably well: the needle moved to the magnetical meridian with a ſlow motion.

EXP. III. I then took the needle uſed in the firſt experiment, and fixed upon it as much cork as was requiſite

juſt

juſt to make it ſink. I placed a point under it, ſo that the needle with the cork was kept a little below the ſurface of the water, bearing upon the point with a very inconſiderable weight. This anſwered nearly as in EXP. II.

EXP. IV. I fixed to the center of a ſmall, but ſtrong, magnetical needle a ſilver wire; to the other extremity of this wire I fixed a ſmall ſteel-working needle, very much hardened. I ſtuck the point of this working needle to the lower extremity of a ſteel magnet, placed in a vertical poſition and highly poliſhed. This whole apparatus was placed in a glaſs cylindrical veſſel, filled with water ſo far as to cover the point of the working needle. The horizontal magnetical needle, thus ſuſpended, was remarkably quick in directing itſelf into the magnetical meridian, and was very eaſily diſplaced out of its direction by the approach of a magnet at a conſiderable diſtance.

The greateſt difficulty I found in this contrivance was, that a little jerk ſhook the point of the working needle from the vertical magnet; and that a heavy magnet could not be ſuſpended from the point of a working needle, but required a tolerably good magnet rounded at its extremity, where it was ſuſpended from the vertical magnet, or a thick piece of iron rounded in the ſame manner, by which means, however, the horizontal

magnet

magnet did not move ſo freely. The firſt difficulty could be eaſily diminiſhed by letting the working needle paſs through a ſmall and ſmooth hole in a glaſs or metallic plate, to controul its vibrating motion, and by adapting to it a ſhoulder, by which it ſhould ſtop upon the glaſs or metallic plate, and from which it ſhould be taken up again, by ſliding the vertical magnet a little down ſo as to touch the point of the working needle, and to lift it up again into its former place. The ſecond difficulty could, I think, alſo be in a great meaſure obviated by employing, inſtead of a common magnetical needle, a thin ſteel tube (of which I will ſpeak by and by) which is only a little ſpecifically heavier than an equal bulk of the liquid. By this means a ſmall vertical magnet could be made to ſupport a large one, even of a conſiderable abſolute weight, of which weight, however, the vertical magnet ſhould ſupport almoſt nothing, it being counter-balanced by the fluid in which it is immerſed.

In the experiment, as I had made it, I could not perceive, that the horizontal magnetical needle was influenced by the vertical magnet, which would probably be the caſe if the vertical one was much out of the vertical line, and too near the horizontal needle.

EXP. V. I adapted to a flat ſtrong magnetical needle two caps, turned one againſt another, ſo that the needle

could be ſupported on either of the caps, and turned with either ſurface upwards. I fixed on one of the flat ſurfaces, on each ſide of the center, a thin glaſs tube, hermetically ſealed, of ſuch a ſize, as made the whole together dip in the water ſo deep that only a ſmall part of the glaſs tubes remained above the ſurface of the water. I then depreſſed the needle entirely under the ſurface of the water by thruſting a metallic point in the cap which was uppermoſt, and fixing this point to the cover of the baſon: thus the needle, kept under water, bore againſt the point only with that ſmall degree of preſſure which the very inconſiderable difference of ſpecific gravity, which it had above an equal bulk of the water, could give it, which did not amount to above a few grains of weight. Thus this needle, loſing nothing of its polarity, loſt very near the whole of the reſiſtance of its weight, and at the ſame time that quivering motion and reſtleſſneſs, which it had in the air, moving ſmoothly in a medium, which could only obſtruct its too great vibrations (almoſt in the ſame way as aſtronomers ſtop the vibrations of their plumb line by hanging the weight in water or oil) without obſtructing (but retarding only) its tendency to the magnetic meridian, liquids preſſing equally on all ſides.

To

To prevent this needle from being ſhook off the point, and wandering about in the baſon, I placed alſo a metallic point underneath, upon which the under cap muſt bear if the needle ſhould by any cauſe deſcend from the upper point, or if I ſhould chuſe to make the whole ſink by increaſing its weight.

Having now found, by experience, that a ſtrong magnetical needle did point to the magnetical meridian near as well under water as in the open air, and that by the reſiſtance of the medium much of its too great verſatility was taken away; I wanted to try, what degree of magnetiſm could be given to a thin ſteel cylindrical tube, as a needle made in this ſhape could be as light as required without being incumbered with cork or glaſs tubes; but finding no ſuch tubes ready made, I tried one made of tin and found it ſuſceptible of a much greater magnetic virtue than I expected, conſidering that the iron plates of which it is made are neither ſufficiently hardened for this purpoſe, nor approach enough to the nature of ſteel; beſides that, being covered with a pewter coating, they cannot be expoſed to the bare friction of a ſteel magnet.

This experiment, however, afforded me a certain proof, that a magnetic needle, made of a thin ſteel tube,

would be ſuſceptible of a ſtrong polarity, ſo as to ſerve for the purpoſe here intended.

But as the two agate caps, if fixed in the middle of the ſubſtance of the tube, would interrupt ſome part of the continuation of the ſteel, and thus leſſen the magnetic power, the points of ſuſpenſion would be better ſoldered or fixed upon the ſurface of the tube itſelf, and the agate caps fixed upon the ſupport underneath and above the tube; ſo that ſuch a needle ſhould be the reverſe (in reſpect to the ſuſpenſion) of the common needles. Another advantage would be derived from fixing the points of ſuſpenſion upon the needle itſelf, *viz.* that by the motion of the fluid in which it ſwims, it would be leſs apt to acquire a too ſtrong waving or undulating motion.

If it ſhould be found difficult to make very thin ſteel tubes properly hardened, a piece of ſteel could be ſcooped out ſo as to conſtitute the half of a tube; the other half could be made of another ſimilar piece, or perhaps better of thin braſs, or any other metal, and they might be ſoldered togther.

It would, perhaps, anſwer the ſame purpoſe if a ſteel magnet was ſhut up in a thin tube of glaſs or ſome metal.

I ſhould

I ſhould think, that there could be no great difficulty to adapt to theſe kind of needles a thin glaſs, metallic, or enamelled plate, ſerving inſtead of the card in the common nautical compaſſes, and to make uſe of them in azimuthal compaſſes.

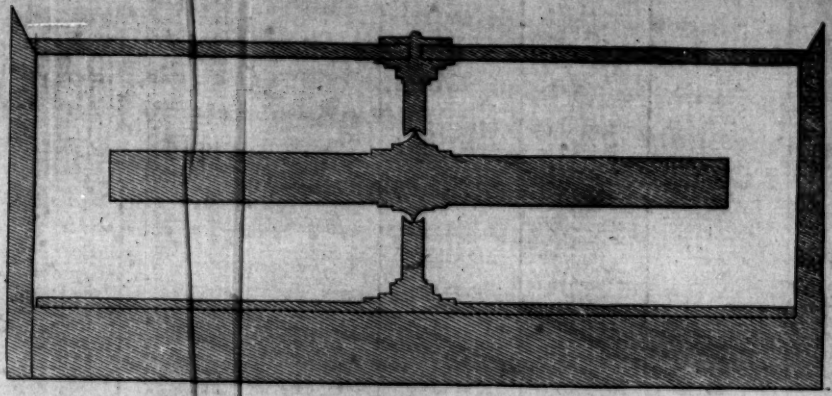
A card, if it is not required to be ſtuck upon the needle, might be fixed to the bottom of the glaſs baſon deſtined for this apparatus.

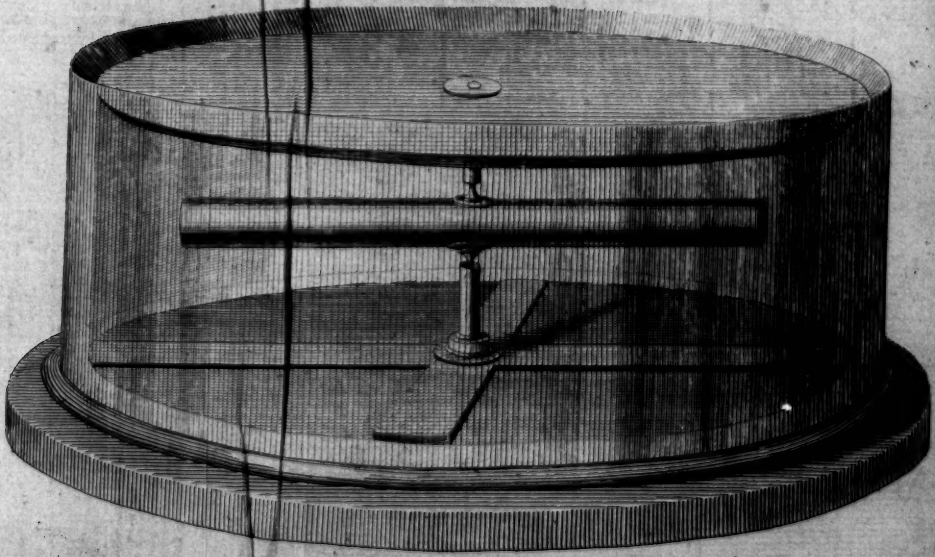
Common water would be, perhaps, the beſt medium for theſe different contrivances, if ſteel was not ſo eaſily ruſted by it, if the needle was covered with ſome varniſh or metal unalterable in water, and if in cold weather it was not ſo apt to freeze; and therefore, I think, ſome of the thinneſt expreſſed oils would anſwer the purpoſe better. Linſeed oil, when it has ſubſided, is very clear and tranſparent, and is not ſubject to freeze, at leaſt in the common cold of our climates; beſides, it increaſes the magnetical power of ſteel, as Dr. KNIGHT found. If it thickens by time, it may be changed.

The glaſs baſon containing ſuch a compaſs ſhould be kept full of the liquid to the cover, to obſtruct undulating motions.

The plate joined to this paper is not made to ſerve as a model of a compleat apparatus, as the drawing was made from

from fancy. It is deſtined only to convey an idea of the invention. I make no doubt but that ingenious artiſts would ſoon greatly improve this rough ſketch, if the contrivance ſhould be found by experience to anſwer at ſea.





XXXV. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1778. *By* Thomas Barker, *Eſquire. Communicated by* Thomas White, *Eſquire, F. R. S.*

Read May 20, 1779.

		Barometer.			Thermometer.						Rain.
					In the Houſe.			Abroad.			
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	29,90	28,16	29,27	42½	32	37	41	18½	32	1,980
	Aftern.				43½	32½	38	44½	21	37	
Feb.	Morn	29,85	28,66	29,33	44½	34	38	45	24	33	0,949
	Aftern.				45	34	39	51	32	38½	
Mar.	Morn.	29,98	28,61	29,32	51	35½	41½	52	24½	36	1,196
	Aftern.				51½	37½	43	59	36	44	
Apr.	Morn.	29,81	28,92	29,31	56	41½	47½	51	30	41	1,037
	Aftern.				58	43	49	65½	40	51	
May	Morn.	29,79	28,94	29,41	61	48½	54	59½	45	50	1,322
	Aftern.				62	49	55½	70	52	61	
June	Morn.	29,86	29,18	29,58	66	53	60	65½	43	55½	2,714
	Aftern.				68	55	61½	80½	57	69	
July	Morn.	29,82	29,02	29,48	70½	59	64½	66	51½	59½	4,103
	Aftern.				73½	61	66½	85	62½	72½	
Aug.	Morn.	30,00	29,10	29,69	67½	56	63	63	42	56	0,391
	Aftern.				70	56	65	76	57	69	
Sept.	Morn.	29,99	28,72	29,54	62½	48	56	55½	32½	46	1,660
	Aftern.				64	50	57	66	44½	58½	
Oct.	Morn.	29,64	28,62	29,31	55	44	48	52	26	39½	4,238
	Aftern.				57	45	49½	61	38½	50	
Nov.	Morn.	29,72	28,63	29,23	52	42	46½	53	30	40½	3,845
	Aftern.				53	43	47	56	38½	46	
Dec.	Morn.	29,23	28,25	29,34	51	40	44	51½	28	40	2,835
	Aftern.				51½	41	45	52½	33	44	
											26,270

This

This ſummer having been hotter than uſual, I here give an abſtract of the hotteſt month in it.

		Barometer.			Thermometer.					
					In the Houſe.			Abroad.		
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean
June 22 to July 21	Morn.	29,82	29,10	29,58	70½	62½	65½	66	54	61
	Aftern.				73½	63	67½	85	70	75

The year began with froſt. The ſharpeſt and longeſt this winter which laſted about a fortnight; but the ſnow never got deep, and the froſt towards the end was much broken, and the winter was not, on the whole, either ſevere or wet. Some windy wet weather followed the froſt; but it was oftener dark, fair, calm, and cold, and frequently ſcarce either froſt or thaw. The ſpring ſeed time was good; at firſt dark and cold, but a fortnight at the end of March and beginning of April was ſunny, fine, and warm, and ſome days quite hot; then it turned cold again, with ſeveral very ſharp, froſty mornings, and ſometimes hail and ſnow.

Towards the end of April it grew mild and growing again; frequent ſmall ſhowers, and ſometimes windy the firſt half of May. As ſummer advanced it was drier and hotter; very much ſo in June and July, being the hotteſt ſummer ſince 1762, if not ſince 1750. The

ground was much burnt, but not ſo much as ſometimes; for two very heavy thunder ſhowers, June 27 and 28, kept the graſs from entirely failing. But as we had ſcarce a ſettled rainy day for half a year, only ſhowers often with thunder, thoſe places where they did not fall were much more burnt than we, which was the caſe in moſt of the South and Eaſt of England, and I believe to the North and Weſt of us they had more rain than we. Many and heavy ſhowers in the twelve laſt days of July made the graſs grow again for a while; but the harveſt was exceeding fine, not a day's hindrance, many finiſhed in Auguſt, and the crops were in general pretty good. At that time the ground burnt again pretty much, there were ſome ſhowers in September, but the ſeaſon was in general dry and calm, and it was upon the whole a very pleaſant year.

Froſty mornings began early, for there were ſome before September was out; and ſoon after the beginning of October it grew wetter, often windy and frequent froſty mornings. This dark, wet ſeaſon continued till toward the middle of December, and grew more ſtormy, but fewer froſty mornings after October. In ſcarce ten weeks there fell near half a year's rain; the dryneſs of the ground carried it off for a good while, but it was very full of wet at laſt. The wheat ſeed-time being early, and

the clays ſowed before the ground got wet, it went very well into the ground, and looks finely in moſt places. The latter part of December was dark, fair, and mild, and in general calm, till the laſt day a violent ſtorm, which ſome ſuppoſed as ſtrong as that in 1703 did a great deal of damage in the North of England, and the Eaſtern part of the South of it; but does not ſeem to have been ſo ſtrong toward the Weſtern parts.

XXXVI. *Extract of a Meteorological Journal for the Year 1778, kept at* Briſtol, *by* Samuel Farr, *M. D.*

Read May 20, 1779.

Months.	Barometer. Higheſt.	Loweſt.	Mean.	Viciſſitude.
January	30.31	28.54	29.37	1. −2 −
February	30.34	29.25	29.81	1. −3 +
March	30.38	29.10	29.77	0.94−1½ −
April	30.25	29.30	29.78	0.30−1 +
May	30.28	29.40	29.56	0.30−½ +
June	30.34	29.65	30.20	0.23−½ +
July	30.29	29.50	29.94	0.24−½ −
Auguſt	30.48	29.73	30.15	0.40−1½ +
September	30.40	29.30	29.80	0.77−1 −
October	30.10	29.18	29.70	0.50−½ −
November	30.26	29.12	29 69	0.60−½ +
December	30.74	28.79	29.86	0.86−1½

+ riſing.
− falling.

Months.	Thermometer in.				Thermometer out.			
	Highest.	Lowest.	Mean.	Vicissitudes.	Highest.	Lowest.	Mean.	Vicissitudes.
				Days.				Days.
January	44	31	36	+ 17–1½	46	30	36	+ 12–1½
February	46	35	40	+ 4– ½	48	32	40⅝	– 8– ½
March	52	39	43	– 9–2½	56	36	44⅛	– 9– ½
April	63	43	51	– 6– ½	70	41	53	– 10–1½
May	66	50	57	+ 4– ½	70	48	58	– 12– ½
June	74	55	61	+ 5– ½	78	53	64	– 12– ½
July	75	61	67	– 4– ½	79	57	68½	+ 7–1½
Auguſt	73	59	65	+ 7–1½	78	61	68	– 7–1
Septemb.	64	47	57	+ 7– ½	67	46	58	– 12–1
October	57	41	49	– 11–1½	59	38	47	– 11– ½
Novemb.	55	43	49	– 4– ½	57	39	46	+ 7– ½
December	54	38	45	– 10–1½	53	28	43⅛	+ 15–1
			51½				52	

An

An abridged table of the WINDS, &c. for BRISTOL, for the Year 1778.

	N	E	W	S	NW	SE	NE	SW	Rain.	Fair Days.	Froſty Days.	Thunder, &c.
Jan.		2		1		9	11½	7½	2.199	7	11	20. 21. 22. S.W.
Feb.	1½	1½	½	½	4½	10	3½	6	0.957	8	6	
Mar.	1	1		2		3½	15	8½	1.406	12	5	
Apr.		½			4½		13	12	0.663	16		30. E.
May	½		½	1½	1½	5	4	18	2.716	20		
June		1		2	1	1	5½	19½	0.972	19		15. N.W. 24. E. 26. S.W. 29. N.E.
July	1			1	2½	1½	3	22	3.003	22		20. N.E. 28. 31. S.W.
Aug.		½		1½	1½	4	8	15½	0.292	22	1	
Sept.	½			½	6	5½	11	6½	1.323	20	3	11. S.W. 27. N.E.
Oct.		½	½	1	6	11½	8½	3	3.987	18	11	
Nov.	1½			½	14½	2	1½	10	5.085	12	5	25. N.W.
Dec.	½	1	3	½	9	5	½	11½	3.300	11	6	
	6½	8	4½	12	51	58	85	140	25.903	187	48	

WEATHER FOR THE YEAR 1778.

January. Cloudy but dry till 4th, with a froſt; rain on 4th and 5th; froſty till 12th; then rainy, except 16th, till 26th; then a froſt till 28th, when it thawed as on 29th; 30th and 31ſt wet.

February. Dry to the 4th and on 5th; then wet to 9th; dry and froſt on 13th to 14th; 16th, 17th, 18th, and 19th, froſty; then wet till 26th; that and 27th had ſome ſnow; 28th fair.

March. Fair to the 6th; 7th cloudy; 8th wet; cloudy but dry to the 12th, and dry to the 20th; wet to 25th; dry to 28th; after that wet to the end.

April.

April. Dry to the 6th, and after that to 9th, and then to the 13th, and after that to 16th; 17th and 18th fair; 19th and 20th ſtormy; then dry and froſty to 27th; 28th cloudy; 29th and 30th wet.

May. The 1ſt dry; then it was wet to the 9th; dry to the 12; then it rained to 19th; was dry to 22d, and afterwards, except 27th, to the end.

June. Wet, except the 3d, till 6th; 7th wet; 8th dry; 9th wet; then dry to 15th, and after that to 24th; 25th dry; 26th, 27th ſtormy; 28th dry; 29th ſtormy; 30th dry.

July. The 1ſt and 3d wet; 2d dry, and to the 16th; then it was wet the reſt of the month.

Auguſt. Some rain on 3d, 6th, 14th, 15th, and 30th; the reſt was quite dry, and chiefly fair.

September. Was dry to the 7th, and on 8th and 9th; then wet to 13th; dry to 16th; 17th wet; then dry to 24th; 25th dry; 26th and 27th wet; 28th, 29th, a froſt; 30th wet.

October. The 1ſt wet; 2d dry; 3d wet; 4th, 5th, dry; 6th, 7th, 8th, wet; then froſty to 19th; after which, except on the 25th, 26th, 29th, it was very wet.

November. The 2d, 8th, 14th, 15th, 20th, 21ſt, and 30th, were dry; the reſt were all wet.

December. 1ſt and 5th froſty; then it rained till 25th, excepting 19 and 21ſt; then a froſt to 28th; then wet to the end.

XXXVII. *A Treatise on Rivers and Canals.* By Theod. Aug. Mann, *Member of the Imperial and Royal Academy of Sciences at* Brussels; *communicated by* Joseph Banks, *Esq. P. R. S.*

Read June 24, 1779.

TO JOSEPH BANKS, ESQ. P. R. S.

SIR,

YOUR election to the Presidency of the first literary and scientific Society in the world, to a chair so long and so gloriously occupied by the great NEWTON; joined to the friendship you have been pleased to honour me with since my being first known to you: has encouraged me to send you something of my composition, as the best way of expressing my sincere respect and attachment to you, and my profound veneration for the illustrious Body which has chosen you for its head. Though various circumstances, by carrying me very early into foreign countries, have made me from my youth almost an

an alien to my native ſoil, and put me in a ſituation which apparently muſt make me ever remain ſo; yet, neither time nor diſtance could ever weaken, much leſs obliterate, my tender attachment to it, or my ardent wiſhes for its welfare.

Theſe conſiderations will, I hope, merit a favourable acceptance from the Royal Society of the following piece, which I have the honour of addreſſing to you; and an indulgent condeſcenſion for its imperfection in every reſpect, and particularly in point of ſtyle. Five and twenty years abſence from my native country, and the neceſſity of converſing during that time in different foreign languages, muſt unavoidably have filled mine, without my being ſenſible of it, with idioms and expreſſions in no wiſe Engliſh.

As to the ſubject I have undertaken to treat on this occaſion, I was guided in the choice thereof by the motive of ſaying ſomething that might be uſeful to my native country. The great number of extenſive and magnificent canals, which have been cut through almoſt every part of England of late years, for the uſe of internal navigation, and which do honour to the public ſpirit of the nation, merit to be conſidered in a ſcientifical as well as in a commercial light. Their waters have their laws of motion different in many caſes from thoſe of

rivers: they are liable to many accidents which the others are not, and of a different nature. These accidents do not become sensible till many years after their construction, and are better prevented in time than remedied when they happen. I have long lived in a country famous for its navigable canals, and have been much employed, under the eyes of the government of it, upon that subject. I only mention this to shew, that I have not undertaken to treat a subject to which I am an utter stranger.

There are, moreover, many considerations concerning the laws of motion in rivers and canals in general, the velocity of their currents in proportion to the quantity of their declivity, and the means of ascertaining therefrom the respective heights of the interior parts of continents, which merit the attention of a natural philosopher. I shall venture to offer my thoughts and observations (some of which, I believe, are new) on all these subjects in the ensuing Dissertation, which I submit entirely to the judgment of the Royal Society, and shall esteem myself happy if I succeed in it, so as to be of any use to my country, and to be able to testify, at the same time, my profound respect and veneration to you, DEAR SIR, and to the illustrious body over which you preside.

SECTION I.

Different uses for which canals are made, with an account of the principal authors who have wrote concerning them.

1. Artificial canals are to be considered in a double light; as facilitating commerce by means of internal navigation, and as preventing inundations by carrying off the too great abundance of water from low and flat countries, such as are Holland, Flanders, &c. In these last named countries they serve at once for both purposes; and it is in this double light that I shall consider them in the ensuing discourse. If canals for draining have sluices upon them, particularly at the end whereby they discharge their waters, as is universally the case in the Low Countries, they differ in no wise from navigable canals: if they have nothing to sustain their waters in them, they are to be considered in every respect as rivers or rivulets, and follow the same laws. It must, therefore, be carefully kept in mind, that whenever I mention canals, I mean those only whose waters are kept up by sluices, and never those without them, which I include, without distinction, under the common appellation of rivers; for

they

they are no more than artificial ones. If I miſtake not, all the navigable canals in England are of the firſt ſort; that is, have their waters kept up, and let off by ſluices. This neceſſary diſtinction will take away all ambiguity from what I have to ſay on canals throughout the following diſcourſe.

2. But that I may fulfil the taſk I have undertaken, it is neceſſary firſt of all to lay down ſuch principles on the nature of rivers and canals in general as have been demonſtrated true both by calculation and experience; to the end, that we may deduce from thence the true laws of motion of their waters, and the quantity of declivity of their beds: for this purpoſe, and becauſe a large volume would hardly ſuffice to compriſe all the demonſtrations of theſe principles, which, conſequently, I am obliged to omit in this treatiſe, it will not be amiſs to mention the principal authors who have treated this ſubject in different ages and countries, in whoſe works the demonſtrations of all the principles I ſhall lay down may be found, if any one doubts the truth of them. Theſe are the following.

SEXTUS JULIUS FRONTINUS, de Aquæ-ductibus Urbis Romæ, cum Notis POLENI, impreſſ. 1722.

JOHN BAPTIST ALEOTTI, Hydrometrician to the Duke of Ferrara, and to Pope CLEMENT the VIIIth.

* Don BENEDICT CASTELLI, Benedictine Abbot, de Mensurâ Aquarum Currentium.

J. B. BARATTERI, de Architettura d'Acque, lib. VI. Piacenza, in folio, 1656.

ALEXANDER BELTINZOLI, of Cremona.

NICOLAUS CABEUS, in Libris Meteorum.

GALILEI GALILEO.

* JOH. BAPT. BALIANI, de Motu Liquidorum.

* JOH. BAPT. RICCIOLI, Geographiæ et Hydrographiæ Reform. libro VI. cc. 29. et 30.

* CLAUDE MILLET DESCHALES, de Fontibus et Fluminibus, à prop. 39. usque ad 56.

VARENNIUS, General Geography, with Dr. JURIN's and Dr. SHAW's Notes, edit. of 1765, vol. I. from page 295 to page 358.

Dr. JURIN, in the Philosophical Transactions, N° 355. page 748. et seq.

MARIOTTE, Traité du Mouvement des Eaux.

VARIGNON, Memoires de l'Academie des Sciences de Paris, pour 1699 et 1703.

* Sir ISAAC NEWTON, Princip. Mathem. lib. II. § 7. page 318 et seq. edit. 1726.

* DANIELIS BERNOUILLI, Hydrodynamica, in quarto, Argentorati, 1738.

* DOMENICHE GUGLIELMINI, della Natura de Fiumi, Bononiæ, 1697, in quarto. Ejuſdem de Menſurâ Aquarum fluentium, Bononiæ, in quarto.

* JOH. POLENUS, de Caſtellis et de Motu Aquæ mixto, Patavii, 1697, 1718, 1723.

* RACCOLTA d'Autori che trattano del Moto dell' Acque, Fiorenza, 1723, 3 vol. quarto, cum fig.

JAC. HERMANNUS, in Phoronomia, cap 10. page 226. et ſeq.

CHRIST. WOLF, Curſ. Mathem. Hydraulicæ, cap. VI. edit. Genevæ, in quarto, 1740.

M. DE BUFFON, ſur les Fleuves, dans ſon Hiſtoire Naturelle, tom. II. p. 38—100. de la 1re edit. en 12mo.

Several Memoirs upon this Subject in the Collection of the Royal Academy of Sciences of Paris, particularly thoſe of M. PITOT, in the volumes for 1730 and 1732.

S'GRAVESANDE, in Elementis Phyſicæ, tom. I. lib. II. cap. 10.

* R. P. LECCHI S. J. Hydroſtatica, Mediolani, 1765. In this excellent work are ſeveral pieces by Father BOSCOVICH upon the ſame ſubject.

* STATTLERI Phyſica, vol. III. p. 232—286. de curſu Fluminum, ejuſque Menſuratione et Directione. Aug. Vindel. 1772, 8 vol. in octavo. This author gives many late obſervations and experiments on the motion and meaſure

measure of currents, as those of ZENDRINI, HIMENII, &c.

Two other authors have lately wrote upon rivers and canals, but their works are not yet come to my hands; to wit,

Father FRISI, an Italian Barnabite, Professor of Mathematics at Milan.

M. DE LA LANDE, of the Royal Academy of Sciences at Paris, who has just published a History in folio, with plates, of all the Navigable Canals in the World that have come to his Knowledge.

Among the above authors, those marked with an asterisk (*) are they who have treated the subject in question with the greatest exactness or most extent; and it is from them chiefly that I shall lay down such principles and laws of action in rivers and canals as regard the subject I have taken in hand. By this means I shall avoid advancing any thing upon so important a matter, but what is founded upon the most certain and exact experiments, and conformable to what are demonstrated to be the real and unalterable laws of nature.

SEC-

SECTION II.

The theory of rivers and canals.

I. DEFINITIONS.

3. A river is a greater or leſſer quantity of water which runs conſtantly, by its own gravity, from the more elevated parts of the earth, towards thoſe which are more depreſſed, in a natural bed or channel open above.

4. If this bed or channel is artificial, and has been dug by hands, it is called *a canal*, of which there are two kinds; thoſe where the channel is every where open, and without ſluices, which I call an *artificial river*; and thoſe where the waters are kept up or let off by the means of ſluices: it is this ſecond ſort which I ſhall call henceforwards by the proper name of *a canal*.

5. *A river is ſaid to perſevere in the ſame ſtate* ſo long as there runs off an equal quantity of water in the ſame time, without any increaſe or diminution, ſo that it remains always at the ſame height in the ſame place. When the circumſtances are different from this, it is ſaid reſpectively that *a river increaſes or diminiſhes*.

6. *A ſection* of the bed of a river or canal is a plane drawn perpendicular to the bottom of the bed and to the

direction of the ſtream of water, and whoſe limits are thoſe of the water itſelf which runs off in that place.

7. I call *ſections of equal velocity*, all thoſe where the water runs with equal velocity; and *ſections of greater or leſſer velocity*, thoſe where the water runs faſter or ſlower reſpectively, and when compared to others.

8. I call *mean velocity of a current or ſtream of water*, that which a river or canal would have, if all the parts thereof, to wit, thoſe of the bottom, the ſides, the middle, and the ſurface of the ſame ſection, ran with an equal velocity, in ſuch a manner that there would paſs juſt as much water in the ſame time by this uniform motion, as there does now actually paſs by the irregular flowing of the ſtream.

II. PROPOSITIONS, *or laws of action in rivers and canals.*

9. The motion of water in rivers proceeds from the ſame principle which produces the deſcent of heavy bodies upon inclined planes.

10. The deſcent of heavy bodies upon inclined planes follows exactly the ſame laws as thoſe obſerved in the deſcent of heavy bodies in a perpendicular line towards the center of the earth; that is,

1ſt, They deſcend by a motion uniformly accelerated.

2dly, The

2dly, The ſpaces, run over by heavy bodies which fall perpendicularly by a motion uniformly accelerated, are in *a duplicate ratio of the times and velocities* reſpectively.

3dly, Theſe ſpaces, in equal times, augment in the ſame ratio as the odd numbers in progreſſion 1, 3, 5, 7, 9, 11, &c.

4thly, Therefore, both the times and the velocities are in a *ſub-duplicate ratio of the ſpaces run over.*

5thly, It is demonſtrated by the principles of mechanics, that the velocity acquired by a heavy body deſcending freely upon an inclined plane, in a given time, is to the velocity which the body would acquire in the ſame time, by falling perpendicularly, as the height of the inclined plane is to its length.

6thly, From whence it follows, that the velocities which bodies acquire in their deſcent upon inclined planes, are *in a direct ratio of the ſquare roots of the quantity of inclination or declivity of the planes.*

11. So that when water flows freely upon an inclined bed, it acquires a velocity, which is always as the ſquare root of the quantity of declivity of the bed.

12. In an horizontal bed, opened by ſluices or otherwiſe, at one or both ends, the water flows out by its gravity alone; and the flowing is quicker or ſlower in a di-

rect ratio of the reſpective heights of the water, by reaſon of the weight of the ſuperior waters upon the inferior.

13. From hence (N° 11, 12.) it follows, firſt, that as much as the declivity of the bed or channel of a river is greater, ſo much alſo will the velocity of the flowing waters be proportionably increaſed.

2dly, As much as the water in an horizontal bed is deeper, ſo much will the velocity of the current be increaſed; and this velocity will diminiſh in proportion to the decreaſing depths of the water in the bed.

3dly, Abſtracting from the reſiſtance cauſed by the bottom and ſides of the bed, as much nearer as the water is to the bottom, ſo much will its motion be accelerated; not only becauſe the inferior waters are more compreſſed by the ſuperior in proportion to their greater depth; but alſo becauſe the inferior ones have a greater declivity than the ſuperior, by reaſon of their greater depth in the bed, where they are more depreſſed with reſpect to the elevation of their common ſource or ſpring. But theſe different velocities of the upper and lower waters in the ſame ſection of the bed (abſtracting from the friction of the bottom and ſides) approximate indefinitely to each other in proportion to the length of the channel, but ſtill without a poſſibility of their ever becoming equal in fact, if they met with no reſiſtance from the bed.

14, There-

14. Therefore, the motion of water flowing freely in an inclined channel, is accelerated by its own weight combined with the quantity of declivity in the bed.

Nevertheleſs, the velocity of waters which flow in an inclined bed, during their actual flowing, is not accelerated by the weight which the inferior waters ſuſtain from the ſuperior ones, in caſe the lower parts have already, by the declivity of the bed, a greater velocity than that which the weight of the ſuperior ones impreſſes upon them. The reaſon of which is, that no body which follows another with a leſſer velocity can act by impulſion upon that which precedes it with a greater velocity, as is the caſe with regard to theſe ſuperior and inferior waters. But the weight of the upper waters begins to accelerate the lower as ſoon as they fall into an horizontal bed, or one that is ſo nearly horizontal as to deſtroy the greater velocity of the lower waters above that of the upper.

15. The velocity of rivers depends ſometimes upon the ſole declivity of their beds; ſometimes alſo upon the ſole gravity of their waters: and if theſe two cauſes ſometimes act together, the effect produced is only the reſpective exceſs of the one above the other. It often happens in the ſame ſection of a river, that the acceleration of velocity in the inferior parts proceeds from the weight of

the upper waters, while that in the upper parts proceeds from the declivity of the bed.

From whence it follows, that in rivers which have little declivity, it is the depth of the waters which contributes moſt to accelerate their current; and in thoſe whoſe beds have moſt declivity, it is the deſcent of gravity upon an inclined plane which has the greateſt ſhare in producing this acceleration.

To find whether the water in a part of a river where the bed is nearly horizontal flows by the velocity acquired in the preceding declivities, or by the compreſſion of the upper waters upon the lower in that place; a pole muſt be thruſt down to the bottom, and held perpendicular to the current of the water, with its upper end above the ſurface: if the water ſwells and riſes immediately againſt the pole, it ſhews that its flowing is by virtue of a preceding declivity: if, on the contrary, the water ſtops for ſome moments before it begins to riſe againſt the pole, it is a proof that it flows by means of the compreſſion of the upper waters upon the lower.

16. The *abſolute height or elevation* of the ſurface of a river which perſeveres in the ſame ſtate (N° 5.) continually decreaſes, as the diſtance in the river from its ſource increaſes; by reaſon that its bed muſt continually incline and tend towards the center of the earth.

17. The

17. The velocity of each particle of water in a regular channel, that is, where the bed is a regularly inclined plane, may be determined by drawing a perpendicular from the particle propoſed to the horizontal curve which paſſes through the ſpring, or that point of the river where the particle in queſtion begins to acquire its velocity. For the velocity which this particle would acquire, in falling freely along the ſaid perpendicular, is the ſame as that which it has acquired in its deſcent along the inclined plane of its bed.

18. So long as *a river perſeveres in the ſame ſtate* (N° 5.) there flows an equal quantity of water in equal times, how unequal ſoever the ſections be through which they flow; and, conſequently, where the ſection of the river is greater, the velocity of the flowing water is leſs; and where the ſection is leſs, the velocity is greater; always in an inverſe proportion. From hence may be deduced the following and other ſimilar propoſitions.

1ſt, Through equal ſections, in equal times, and with equal velocity, there muſt flow equal quantities of water.

2dly, Through equal ſections, in equal times, but with unequal velocities, the quantities of water which flow, are in a direct ratio of the reſpective velocities.

3dly, Through

3dly, Through unequal ſections, in equal times, and with equal velocities, the quantities of water which paſs are in a direct ratio of their reſpective ſections.

4thly, Through unequal ſections, and with unequal velocities, the quantities of water which flow in equal times, are in a combined ratio of the ſections and mean velocities (N° 8.) together.

In a word, the ſections of the bed, the mean velocities, the times of flowing, and the quantity which flows, are univerſally in a combined ratio together; and this combination is what is called *the momentum of a river*; and this *momentum of the ſame flowing water is univerſally equal.*

19. From hence may eaſily be deduced the principles for calculating the quantity of diminution of the water in a lake, pond, or veſſel, by any determinate flowings whatſoever: for as the ſurface of the lake, &c. is to the ſection of the current which carries off the waters; ſo is the mean velocity of the current in this ſection to the decreaſe of the waters in the lake, &c. and *vice verſâ.*

III. *On*

III. *On the nature of rivers and flowing waters.*

20. Rivers contain divers inherent cauſes of the acceleration of their motion.

1ſt, Their ſprings are either in mountains or on high grounds, and it is by the deſcent of the waters from theſe elevations that they acquire a velocity and acceleration of motion ſufficient to ſuſtain and propagate it through the reſt of their courſe.

2dly, The coheſion of the particles of a fluid, *in a bed ever ſo little inclined*, is a ſecond cauſe of acceleration of motion in the fluid; becauſe, by their mutual attraction, thoſe particles which begin firſt to flow draw after them thoſe which are contiguous, theſe the following, and ſo on *ad infinitum*.

3dly, Moreover, where a river, by flowing in a bed nearly horizontal, has loſt a great part of the velocity which it had acquired in the preceding declivities, and the bed by this means is become large and ſhallow, which conſequently again augments the ſlowneſs of the current; it may, however, recover a part of its velocity, even in the ſame horizontal bed, by augmenting the depth thereof, and diminiſhing its breadth; for by this means the weight of the ſuperior waters upon the inferior

rior is increaſed, and conſequently the velocity of the whole is augmented (N° 13. 18.). In the ſame manner a junction of rivers in the ſame bed, by excavating and deepening it, augment the velocity of the common current, as we ſhall ſhew more particularly hereafter.

21. On the other hand, flowing waters meet with many powerful cauſes of reſiſtance to their motion, which tend continually to diminiſh their velocity. Such are the following:

1ſt, The attraction and continual friction of the bottom and the ſides of the bed, contribute greatly towards retarding the motion of the water.

2dly, The ſame effect is produced likewiſe by the many obſtacles which they meet with in their way; ſuch as inequalities in the bottom and ſides of the channel, banks of ſand and mud, rocks, trunks of trees, and other ſuch things.

3dly, The many windings and angles made in their courſe, which produce ſo much the more reſiſtance and hindrance to the motion of the water, as the courſe varies more and oftener from a right-line.

4thly, The diminution of their declivity the farther they recede from their ſprings; this being generally the leaſt towards their mouths, which are for the moſt part in extenſive plains.

Finally,

Finally, the natural cohesion of the particles of water *in an horizontal bed* contributes to retard its motion precisely by the same force which contributes to accelerate it *in an inclined bed.* By diminishing or taking away the above obstacles to the free motion of water in rivers and canals, the velocity of their currents will be increased in the same proportion, and thereby also all the dangers and ravages of inundations may be prevented, as we shall shew hereafter.

But if some or all of these causes, in a greater or lesser degree, did not exist in rivers of considerable depth and declivity, it is demonstrated (a), that the velocity of their currents would be accelerated to twelve, fifteen, and, in some cases, even to twenty times more than it is at present in the same rivers, whereby they would become absolutely unnavigable.

22. The waters in a river or open canal have their motion accelerated, so long as the effects proceeding from gravitation, declivity, depth, in a word, so long as *the sum of accelerations surpasses the sum of resistances.* When these different sums become equal to each other, the motion of the water is neither accelerated nor retarded, but remains equal, till something anew destroys the *equilibrium.*

(a) Vide LECCHI, Hydrostat. et STATTLERI, Physic. tom. III. p. 252.

When the ſum of reſiſtances and cauſes of retardation is greater than the ſum of accelerating cauſes, the velocity of the river is diminiſhed in proportion to the exceſs.

23. The *percuſſion* of the waters of a river againſt an obſtacle which is oppoſed to their motion, is the action of the waters ſtriking againſt that obſtacle; and the principles for calculating the quantity of this percuſſion, or the effects which any obſtacles whatever produce in the motion of rivers, by known forces, and in determinate times, are as follows:

1ſt, The percuſſion of the water of a river againſt any obſtacle whatever is univerſally in a compound ratio of the quantity of the plane or planes which the obſtacle oppoſes to the current of water, of the ſine of the angle of incidence which the direction of the current makes with theſe planes, and of the ſquare of the velocity of the ſaid current.

2dly, The reſiſtance, therefore, which the bed of a river oppoſes to its current from any particular obſtacles in it, is in a compound ratio of the magnitude and ſituation of the planes of theſe obſtacles, together with the ſquare of the current's velocity in the place where thoſe obſtacles are found.

3dly, The accelerating force of a river, or that by which it ſurmounts the reſiſtance of its bed in any one

place, compared to that in another, is in a compound ratio of the maſs of water and of the velocity of the current in thoſe places reſpectively.

24. In different parts of the ſame river, the velocity of the current is greater in a direct proportion of the greater declivity of the bed; becauſe the relative gravity of the flowing particles augments in that ratio.

25. But in the ſame ſection of a river, the ſuperior parts, and thoſe which are fartheſt from the bottom and the ſides, will continue their courſe by the ſole cauſe of the declivity of their bed, *how little ſoever it be*; becauſe theſe waters not being retarded by the friction of the bottom and ſides of the bed, or hardly by any other obſtacle whatever, *the leaſt poſſible deviation from a level will produce a current.* But the waters at the bottom of a river, both becauſe of their friction againſt it, and of the irregularities which are almoſt every where found in it, will loſe that little motion which a very ſmall declivity can give them, and their motion in that caſe will be produced alone by the compreſſion of the ſuperior waters upon them.

The inferior waters which thus acquire their motion from the weight of the ſuperior ones upon them, communicate reciprocally a part of their motion, by means of the natural coheſion of the particles together (N° 20.)

to the ſuperior ones, which in an horizontal bed, without this cauſe, would have no other motion than that which is impreſſed upon them by the impulſive force of the waters deſcending from their elevated ſprings.

From whence it appears, that the ſuperior and inferior waters in a river communicate reciprocally a part of their motion to each other; but this can never go beyond a certain point or *maximum*, which is always proportionable to the *momentum* of the river in that place (N° 18.).

It follows from hence, that the greateſt velocity of a river, running in a right-line, is in *the center of its ſection* (N° 6.); that is to ſay, in that point which is the fartheſt poſſible from the ſurface of the water and from the bottom and ſides of the bed, all taken together. This part has the advantage of one half of the depth of water preſſing upon it, and it is exempt from the friction of the bottom and ſides of the bed which are there overcome and vaniſh by the perpendicular compreſſion.

On the contrary, the leaſt velocity of the water is at the bottom and ſides of the bed, becauſe it is there that the reſiſtance produced by friction is greateſt, from whence it is communicated to the other parts of the ſection in *an inverſe duplicate proportion of the diſtances from the bottom and ſides combined together*, until it becomes a negative

negative quantity, where the effect vaniſhes, or is reduced to nothing.

26. The beſt and moſt ſimple method of meaſuring the velocity of the current of a river or open canal, that I know of, is the following:

Take a cylindrical piece of dry, light wood, and of a length ſomething leſs than the depth of the water in the river: round one end of it let there be ſuſpended as many ſmall weights as may be neceſſary to keep up the cylinder in a perpendicular ſituation in the water, and in ſuch a manner that the other end of it may juſt appear above the ſurface of the water. Fix to the center of that end which appears above water a ſmall and ſtraight rod, preciſely in the direction of the cylinder's axis; to the end, that when the inſtrument is ſuſpended in the water, the deviations of the rod from a perpendicularity to the ſurface of it may indicate which end of the cylinder advances the faſteſt, whereby may be diſcovered the different velocities of the water at different depths; for if the rod inclines forwards according to the direction of the current, it is a proof that the ſurface of the water has the greateſt velocity; but if it inclines back, it ſhews that the ſwifteſt current is at the bottom; if it remains perpendicular, it is a ſign that the velocities at the ſurface and bottom are equal.

This

This inſtrument being placed in the current of a river or canal receives all the percuſſions of the water throughout the whole depth, and will have an equal velocity with that of the whole current *from the ſurface to the bottom* at the place where it is put in, and by that means may be found, both with eaſe and exactneſs, the mean velocity of that part of the river for any determinate diſtance and time.

But to obtain the mean velocity of the whole ſection of the river, the inſtrument muſt be put ſucceſſively both in the middle and towards the ſides, becauſe the velocities at thoſe places are often very different from each other. Having by this means found the *difference of time required for the currents to run over an equal ſpace*; or, *the different diſtances run over in equal times, the mean proportional* of all theſe trials, which is found by dividing the common ſum of them all by the number of trials, *will be the mean velocity of the river or canal.*

If it be required to find the velocity of the current only at the ſurface, or at the middle, or at the bottom, a ſphere of wood, of ſuch a weight as will remain ſuſpended in equilibrium with the water at the ſurface or depth which we want to meaſure, will be better for the purpoſe than a cylinder, becauſe it is only affected by the water

water of that ſole part of the current where it remains ſuſpended.

It is very eaſy to guide both the cylinder and the globe in that part which we want to meaſure, by means of two threads or ſmall cords, which two perſons muſt hold and direct, one on each ſide the river; taking care at the ſame time neither to retard nor accelerate the motion of the inſtrument.

Several other methods have been invented for determining the velocity of the currents of rivers and canals, which may be ſeen in moſt of the authors enumerated in the beginning of this eſſay (N° 2.)

IV. *Application of the preceding laws of the acceleration and retardation of currents to rivers and canals in general, from whence are deduced the various means of preventing or remedying the defects and inconveniencies which muſt neceſſarily happen to them in a ſeries of years.*

27. By combining together all we have ſaid hitherto upon the nature and theory of motion in rivers, and particularly in the articles 13. 18. 20. 21. and 23 it follows evidently, that *the deeper the waters are in their bed in proportion to its breadth, the more their motion is accelerated;*

celerated; ſo that *their velocity increaſes in an inverſe ratio of the breadth of the bed, and alſo of the greatneſs of the ſection*; from whence are deduced the two following univerſal practical rules:

1ſt, To augment the velocity of water in a river or canal, without augmenting the declivity of the bed, we muſt *increaſe the depth and diminiſh the breadth of its bed.*

2dly, But to diminiſh the velocity of water in a river or canal, we muſt, on the contrary, *increaſe the breadth and diminiſh the depth of its bed.*

The above propoſition is perfectly conformable to obſervation and experience; for it is conſtantly ſeen, that the current is the ſwifteſt where the waters are deepeſt and the breadth of the bed the leaſt; and that they flow ſloweſt where their depth is the leaſt and the breadth of the bed the greateſt. "The velocity of waters," ſays M. DE BUFFON (b), "augments in the ſame proportion as "the ſection of the channel through which they paſs "diminiſhes, *the force of impulſion from the back-waters "being ſuppoſed always the ſame.* Nothing," continues he, "produces ſo great a diminution in the ſwiftneſs of "a current as its growing ſhallow; and, on the contrary, "the increaſe of the volume of water augments its

(b) Hiſt. Nat. tom. II. p. 53. 60. edit. in 12mo.

" velocity more than any other cauſe whatever." The celebrated WOLF, in his Hydraulics (c), aſſures us, that " it is a conſtant and univerſal practice, for accelerating " the current of waters, to deepen the bed, and at the " ſame time to render it narrower."

28. When the velocity which a river has acquired by the elevation of its ſprings and the impulſe of the back-water, is at laſt totally deſtroyed by the different cauſes of reſiſtance which we have enumerated above (N° 21.) becoming equal or greater than the firſt, the bed and current at the ſame time being exactly horizontal, nothing elſe remains to propagate the motion, except *the ſole perpendicular compreſſion of the upper waters upon the lower, which is always in a direct ratio of their depth.* But this neceſſary reſource, this remaining cauſe of motion in rivers, augments in proportion as all the other diminiſh, and as the want of it increaſes: for as the waters of rivers in extenſive plains loſe the acceleration of motion acquired in their deſcent from their ſprings, their quantity accumulates in the ſame bed by the junction of ſeveral ſtreams together, and their depth increaſes in conſequence thereof. This junction and ſucceſſive accumulation of many ſtreams in the ſame bed, which we ſee univerſally in a greater or leſs degree in all rivers throughout the known world, and which is ſo abſolutely

(c) N. 224.

neceſſary to the motion of their waters, can only be attributed, ſays Signor GUGLIELMINI [d], to the infinite wiſdom of the ſupreme Author of Nature.

29. The velocity of flowing waters is very far from being in proportion to the quantity of declivity in their bed: if it was, a river whoſe declivity is uniform and double to that of another, ought only to run with double the ſwiftneſs when compared to it; but in effect it is found to have a much greater, and its rapidity, inſtead of being only double, will be triple, quadruple, and ſometimes even more: for its velocity depends much more on the quantity and depth of the water, and on the compreſſion of the upper waters on the lower, than on the declivity of the bed. Conſequently, whenever the bed of a river or canal is to be dug, the declivity muſt not be diſtributed equally throughout the whole length; but, to give a ſwifter current to the water, the declivity muſt be made much greater in the beginning of its courſe than towards the end where it diſembogues itſelf, and where the declivity muſt be almoſt inſenſible, as we ſee is the caſe in all natural rivers; for when they approach near the ſea their declivity is little or nothing, yet they flow with a rapidity which is ſo much greater, as they contain a greater volume of water; ſo that in

(d) Della Natura de Fiumi.

great

great rivers, although a large extent of their bed next the ſea ſhould be abſolutely horizontal, and without any declivity at all, yet their waters do not ceaſe to flow, and to flow even with great rapidity, both from the impulſion of the back waters, and from the compreſſion of the upper waters upon the lower in the ſame ſection.

30. Whoever is well acquainted with the principles of the higher geometry, will eaſily perceive that it would be no difficult matter ſo to dig the bed of a canal or river, that *the velocity of the current ſhould be every where equal.* It would be only giving it the form of a curve along which a moving body ſhould recede from a given point, and *deſcribe ſpaces every where proportional to the times*, allowance being made therein for the quantity of effect of the compreſſion of the upper waters upon the lower. This curve is what is called the *Horizontal Iſochronic*, being the flatteſt of an infinity of others which would equally anſwer the problem where fluids were not concerned. Upon theſe curves may be ſeen LEIBNITZ, HUYGHENS, and the two BERNOULLI's, who were the firſt that determined and analyſed them, and alſo many ſucceeding geometricians, if any one is deſirous to occupy himſelf in ſuch ſpeculations as are more curious than uſeful, which is not my purpoſe in this treatiſe.

31. Notwithſtanding all we have ſaid concerning the neceſſity of augmenting the depth of a river in a greater proportion than its breadth, if we would accelerate its current; yet it is certain, that this can only be done to a certain point, without deſtroying that equilibrium which ought to reign between the depth and the breadth of the ſection of the ſtream, and thereby putting the river into a ſtate of continual violence, which will inceſſantly exert itſelf to the deſtruction of the banks and wiers made to keep it in, and that action will always exert itſelf in a direct ratio of the greater or leſs want of equilibrium, as it would be eaſy to demonſtrate by the principles of hydraulics. Theſe ſame principles give likewiſe the juſt proportions of this equilibrium between the perpendicular and lateral compreſſion of the water in any river or canal whatſoever, which vary in an inverſe proportion, according to the different degrees of the declivity and velocity of the current; and in a direct one of the greater or leſs coherence and hardneſs of the ſubſtances which compoſe the bed. Rivers which flow in beds compoſed of homogeneous matter of little conſiſtency, ſuch as ſand, &c. are always more broad than deep, when compared to thoſe which run in beds of matter of greater tenacity. It is manifeſt, that the equilibrium here ſpoken of is real,

becauſe rivers remaining in the ſame ſtate only widen their beds to a certain pitch which they do not ſurpaſs.

32. M. DE BUFFON remarks, "That people accuſ-"tomed to rivers can eaſily foretell when there is going "to be a ſudden increaſe of water in the bed from floods "produced by ſudden falls of rain in the higher countries "through which the rivers paſs. This they perceive by "a particular motion in the water, which they expreſs "in their dialect, by ſaying that *the river's bottom moves*; "that is, the water at the bottom of a channel runs off "faſter than uſual; and this increaſe of motion at the "bottom of the river always announces a ſudden in-"creaſe of water coming down the ſtream. Nor does "their opinion therein," continues the ſame author, "ſeem to be ill-grounded on the nature of things; for "the motion and weight of the waters coming down, "though not yet arrived, muſt act upon the waters in "the lower parts of the river, and communicate by im-"pulſion part of their motion thereto; ſince a canal or "river contained in its bed is to be conſidered in ſome "degree as a column of water contained in a long tube, "where the motion is communicated at once throughout "the whole length." In a river or canal, open above, it is only communicated to a certain diſtance; that is, as far as the impulſive force of the new increaſe and ſuperior rapidity

rapidity of the back-waters acts upon the ſtream, which will always be as far as till this force is gradually, and at laſt wholly, deſtroyed by the ſuperior gravitation of the ſuper-incumbent waters in the ſtream. Something of the ſame kind happens when a very great additional weight comes ſuddenly upon the ſurface of a river or canal; for inſtance, by the launching of a ſhip or of ſeveral boats together upon it. Theſe cauſes increaſe the velocity of the water in the lower parts of the bed, and moreover retard its motion at the ſurface, which effect may properly be called *making the river's bottom move.* For the ſame reaſon, the increaſe of weight of the waters in a ſudden flood, as well as the increaſe of their impulſive force, muſt contribute to produce this effect, and, by increaſing the motion in the bottom of the river, may hinder, for ſome ſpace of time, the ſtream from ſenſibly riſing in the bed.

33. All obſtacles whatever in the bed of a river or canal, ſuch as rocks, trunks of trees, banks of ſand and mud, &c. muſt neceſſarily hinder proportionably the free running off of the water; for it is evident, from what we have ſaid, that the waters ſo far back from theſe obſtacles, until the horizontal level of the bottom of the bed becomes higher than the top of the obſtacles, muſt be intirely kept up and hindered from running off in pro-

portion

portion thereto (N° 23.). Now as the waters muſt continue to come down from their ſources, if their free running off is hindered by any obſtacles whatever, their relative height back from them muſt neceſſarily be increaſed until their elevation, combined with the velocity of their current proceeding from it, be arrived to ſuch a pitch at the point where the obſtacles exiſt, as to counterbalance the quantity of oppoſition or impediment proceeding from thence, which frequently does not happen until all the lower parts of the country round about are laid under water.

34. Now it is certain from all experience, that the beds of rivers and canals in general are ſubject to ſome or others of the obſtacles above mentioned. If rocks or trees do not bar their channels, at leaſt the quantity of ſand, earth, and mud, which their ſtreams never fail to bring down, particularly in floods, and which are unequally depoſed according to the various windings and degrees of ſwiftneſs in the current, muſt unavoidably, in courſe of time, fill up, in part, different places in the channel, and thereby hinder the free running off of the back waters. This is certainly the caſe, more or leſs, in all rivers, and in all canals of long ſtanding, as is notorious to all thoſe well acquainted with them. Hence, if theſe accidents are not carefully, and with a conſtant

conſtant attention prevented, come inundations, which ſometimes lay waſte whole diſtricts, and ruin the fineſt tracts of ground, by covering them with ſand: hence rivers become unnavigable, and canals uſeleſs, for the purpoſes for which they were conſtructed. Canals, in particular, by reaſon that their waters for the moſt part remain ſtagnant in them, are ſtill more liable than rivers to have their beds fill up by the ſubſiding of mud, and that eſpecially for ſome diſtance above each of their ſluices; inſomuch, that if continual care is not taken to prevent it, or remedy it as often as it happens, they will ſoon become incapable of receiving and paſſing the ſame veſſels as formerly. Nay, the very ſluices themſelves, if the floors of their bottoms are not of a depth conformable to the bed of the canal, will produce the ſame accidents as thoſe we have been ſpeaking of; for if they are placed too low, they will be continually filling up with ſand or mud; if too high, they have the ſame effect as banks or bars in the bed of a river, that is, they hinder all the back-waters under their level from running off, and ſoon fill up the bed to that height by the ſubſiding of mud. This effect is much accelerated by the ſhutting of the lower ſluices, which makes a great volume of water reflow back to thoſe next above them, till the whole is filled and becomes ſtagnant. Now it is evident,

that

that this ſtate of things muſt contribute far more to the ſubſidency of mud and all other matters brought down by the waters in canals, than can be the caſe in rivers whoſe currents conſtantly flow.

35. I do not ſuppoſe that theſe inconveniencies can have yet manifeſted themſelves by any very ſenſible effects in the many new canals and ſluices lately conſtructed in England; but as the ſame cauſes do not ceaſe to act more or leſs every where, the effects which neceſſarily follow from them will likewiſe become more and more ſenſible, unleſs continual care be taken to prevent them. The waters of all rivers and canals are from time to time muddy: their ſtreams, particularly during rains and floods, carry along with them earth and other ſubſtances which ſubſide in thoſe places where their currents are the leaſt, whereby their beds are continually raiſed: ſo that the ſucceſſive increaſe of inundations in rivers, and of unfitneſs for navigation in canals, when they are neglected and left to themſelves, is a natural and neceſſary conſequence of the ſtate of things, which no intelligent perſon can be at a loſs to account for; and yet I have known whole countries remain in this habitual ſtate of negligence to their very great detriment.

36. Having thus ſhewn the principal accidents which rivers and canals are liable to, with the cauſes of them, I

ſhall proceed to point out the moſt efficacious methods of preventing them, or at leaſt of diminiſhing their effects. Perhaps it would have been more proper to have deferred doing this till I ſhould have ſaid all I have to ſay upon the nature of rivers and canals: however, I ſhall forego the more ſcientific order of things, for the ſake of bringing the means of remedying the accidents and inconveniences which happen, nearer to the cauſes that produce them, whereby their connexion and efficacity may be better judged of. For this end, I ſhall here lay down, briefly and in general terms, the methods moſt proper for the purpoſe in queſtion. They flow immediately from the principles already laid down in this eſſay, and do not need many words to make them compleatly underſtood.

37. A work of this kind, if it is properly conducted, muſt be begun at the lower end of the river or canal; that is, to ſay, at that end where their waters are diſcharged into the ſea, or where they fall into ſome other greater river or canal, from whence their waters are carried off without farther hindrance. If it is a river whoſe bed, by being filled up with mud, ſand, or other obſtacles, and by being otherwiſe become irregular in its courſe, is thereby often ſubject to inundations, and incapable of internal navigation, the point, from which

the work muſt be begun and directed throughout all the reſt of the channel, is from the loweſt water-mark of ſpring tides on the ſhore at the mouth of the river; or even ſomething below it, if it can be done; though this part will ſoon fill up again by the ſand, mud, &c. which the tides ceaſe not to roll in.

If it is a canal whoſe bed is be to dug anew, or one already made, which is to be cleaned and deepened from the ſea ſhore or ſome large river back into the country, and where no declivity is to be loſt; as is the caſe in all flat countries: the work muſt be begun, and the depth of the whole channel directed, from the low water-mark of ſpring tides, if the mouth is to the ſea, or from ſuch a depth in the channel of the river, if the canal falls into one, that there may be ſuch a communication of water from the canal to the river, in all ſituations of the current, as may let boats freely paſs from one to the other. This, of courſe, muſt alſo direct the depth of the floor of the laſt ſluice towards the mouth of the canal, be it to the ſea or into a river. If the bottom or floor of a ſluice already conſtructed be too low, it will ſoon fill up with ſand or mud, and thereby hinder the gates from opening, unleſs it be continually cleaned out; if, on the contrary, this floor be too high, and in a canal whoſe natural declivity is too little for the free current of the water, as is

generally the caſe in Holland and Flanders, all depth of the bed of the canal below the horizontal level of the bottom of the ſluice will ſerve to no manner of purpoſe, either for navigation, or for carrying off the back-waters, but will ſoon fill up with mud, in ſpite of all means uſed to the contrary, except that of digging it continually anew to no manner of purpoſe; as is evident from the reaſons given above (N° 33. 34.).

38. Setting off from this determinate point, at the mouth of a river, or at the bottom of the laſt ſluice upon a canal, which are to be cleaned and deepened; the work muſt be carried on, in conſequence, uniformly throughout their whole courſe backwards into the country as far as is found neceſſary for the purpoſes intended. This is to be done after the following manner:

1ſt, One muſt dig up and carry away all irregularities in the bottom and ſides of the bed, ſuch as banks of ſand and mud, rocks, ſtumps or trunks of trees, and whatever elſe may cauſe an obſtacle to the regular motion of the water, and to the free paſſage of veſſels upon it.

2dly, If the declivity of the bed ſhould be ſtill too little to give a ſufficient current to carry off the water as often and as faſt as is neceſſary, the whole bed itſelf muſt be regularly deepened, and what is dug out from the bottom muſt be laid upon the ſides, to render it narrower in

proportion to its depth. The reaſon of this is evident from all that has been ſaid.

3dly, Wherever the banks are too low to contain the ſtream in all its ſituations, they muſt be ſufficiently raiſed; which may be conveniently done with what is dug out from the bed: and the whole being covered with green turf will render theſe banks firm and ſolid againſt the corroſion of the water. It is proper at all times to lay upon the banks what is dug from the bed, by which they are continually ſtrengthened againſt the force of the current.

4thly, It is often neceſſary to diminiſh the windings and ſinuoſities in the channel as much as poſſible, by making new cuts whereby its courſe may approach towards a right line. This is a great reſource in flat countries ſubject to inundations; becauſe thereby all the declivity of a great extent of the river, through its turns and windings, may be thrown into a ſmall ſpace by cutting a new channel in a ſtraight line; as may generally be done without obſtacle in ſuch countries as I am ſpeaking of, and hereby the velocity of the current will be very greatly augmented, and the back-waters carried off to a ſurprizing degree, as is evident from what is ſaid above in N° 29.

5thly,

5thly, Wherever there is a confluence of rivers or canals, the angle of their junction muſt be made as acute as poſſible, or elſe the worſt of conſequences will ariſe from the corroſion of their reſpective ſtreams; what they carry off from the ſides will be thrown into irregular banks in the bottom of the bed. This acute angle of junction may always be procured by taking the direction at ſome diſtance from the point of confluence.

6thly, Wherever the ſides or banks of a river are liable to a more particular corroſion, either from the confluence of ſtreams, or from irremediable windings and turns in the channel, they muſt be ſecured againſt it as much as poſſible by *weirs:* for this corroſion not only deſtroys the banks, and alters by degrees the courſe of the river, but alſo fills up the bed, and thereby produces all the bad effects we have ſpoken of above in N° 33. 34. &c.

7thly, But the principal and greateſt attention in digging the beds of rivers and canals muſt be had to the *quantity and form of their declivity.* This muſt be done uniformly throughout their whole extent, or ſo much of it as is neceſſary for the purpoſes in hand, according to the principles laid down above (in N° 29 and 30.) Conformable thereto, the depths of their beds, and of the floors of their ſluices, at the mouths whereby they diſ-

charge

charge their waters, being fixed according to what we have ſaid in N° 37. the depth of the reſt of the beds, and the quantity of declivity therein, muſt be regulated in conſequence thereof, ſo as to increaſe regularly the quantity of declivity in equal ſpaces the farther we recede from their mouths, and proceed towards their ſources or to the part where the regular current is to take place.

If the depth and volume of water in a river or canal is conſiderable, it will ſuffice, in the part next the mouth, to allow one foot perpendicular of declivity through ſix, eight, or even, according to DESCHALES [d], ten thouſand feet in horizontal extent; at moſt it muſt not be above one in ſix or ſeven thouſand. From hence the quantity of declivity in equal ſpaces muſt ſlowly and gradually increaſe as far as the current is to be made fit for navigation; but in ſuch a manner, as that at this upper end there may not be above one foot of perpendicular declivity in four thouſand feet of horizontal extent. If it be made greater than that in a regular bed containing a conſiderable volume of water, the current will be ſo ſtrong as to be found very unfit for the purpoſes of navigation, as will appear hereafter, when I come to inveſtigate the quantity of declivity in ſeveral rivers, the degree of ſwiftneſs of whoſe currents is well known.

(d) De Fontibus et Fluviis, prop. 49.

39. If

39. I dare boldly affirm, from the certain principles of hydrodynamics laid down in this eſſay, that if the above mentioned things (N° 37. 38.) were carried into execution in a proper manner; the velocity of currents and the acceleration of motion of the waters in rivers, and in canals when their fluices are open, might be increaſed to any degree that can be required for opening their beds, and for preventing inundations during great rains or ſudden floods: by carrying off more ſwiftly the great acceſſion of water which then takes place. It would not be difficult, by theſe means, to increaſe the velocity of the current to double and triple what it is in rivers and canals, whoſe beds for a long ſpace of time have been left to themſelves. There is not, perhaps, a country on earth but what might be freed from inundations by theſe means. But it may be objected, that if all I have adviſed was put in execution, even in the flatteſt countries, the currents of rivers (for canals ſhut up with fluices are here out of the queſtion) would become incommodious, if not unfit, for navigation, eſpecially againſt their ſtreams. This objection would be of weight if it was not evident that the various means which I have pointed out may be executed in whole or in part, to a certain degree, and no farther than neceſſary for the purpoſes required. But, as it is certain that a ſtrong and regular

regular current in a river is the beſt of all means for keeping it open and deep, and for preventing the formation of banks in the bed by the ſubſidency of mud, &c. which it does not allow time to precipitate; I leave it to be conſidered, whether it is better to have a free and open navigation ſomething incommoded by the ſtrength of the current, or to have ſoon no navigation at all, without repeatedly digging the bed anew.

40. I ſhall not here enter into the mechanical part of the methods of digging and cleaning canals, rivers, and ſea ports, or into any deſcription of the machines and inſtruments neceſſary for that purpoſe. The ſubject would lead me much too far: beſides all theſe things may be found much at length in moſt of the authors who have wrote upon hydraulic-architecture, ſuch as BARATTERI, CORNELIO MEYERI, GUGLIELMINI, and *a notorious anonymous French plagiary*, who has taken from MEYERI, without ever naming him, almoſt all that is contained in his book, publiſhed at Paris in 1693, and at Amſterdam in 1696, in octavo, under the title of *Traité des Moyens de rendre les Rivières navigables*. But the author who has treated this ſubject with the greateſt care, and moſt at length, is the celebrated BELIDOR, in his *Architecture Hydraulique*, 4 vol. in quarto. To theſe may be added a late memoir of M. FORFAIT of

Rouen, vice-architect of the French navy, which gained the prize of the Royal Academy of Sciences and Belles Lettres of Mantua, for having given the beſt ſolution of a problem propoſed by that Society in 1776, in the following terms: "*To indicate the beſt and cheapeſt method* "*of freeing navigable canals from banks of ſand and* "*earth formed in their beds which render them too ſhal-*"*low.*" This piece, printed at Mantua, by Pazzoni, in 1778, contains ſixty-three pages in quarto, and is divided into two parts; the firſt contains the means of preventing the formation of banks in navigable canals; and the ſecond offers divers methods for remedying them when they are already formed. For this purpoſe the author propoſes ſix different machines of his own invention: the firſt may be employed in rivers near the ſea, and ſubject to the ebb and flow of the tides; the ſecond may be uſed in thoſe where the waters are always nearly of the ſame height and velocity; the third and fourth are to be uſed in thoſe places where the violence of the currents corrode the beds; and the two laſt ſerve to break up the banks of ſand or earth formed in the bottom, and to carry off all heterogeneous bodies ſunk in the river, which cauſe an obſtacle to the current. It would be difficult to give a juſt idea of theſe machines without the help of the ſix plates which accompany the piece;

piece; but as this production of a foreigner has been crowned in Italy, the country of all others in which, from all antiquity, the ſcience of rivers and canals has been moſt cultivated, we cannot well doubt of its merit, or that it is worthy of a tranſlation into our own language.

V. *Other conſiderations on the nature of rivers and inundations.*

41. Rivers flowing along plains, as well as through vallies, have naturally their beds in the loweſt part of the ground comprized between the oppoſite hills or mountains: nevertheleſs, the ſurface of the water of a river in the midſt of a plain is often higher than the ſurface of the grounds adjacent to the banks of the river. This proceeds from the continual ſubſiding of the mud, &c. brought down by the ſtream during floods; the waters in that caſe uſually overflowing the banks ſpread themſelves over the plain, where they loſe a great part of the ſwiftneſs of their current, which contributes greatly to the ſubſiding of the mud they contain; ſo that the farther they flow upon the plain, the clearer they grow, and the leſs remains to ſubſide. From hence the greateſt precipitation of mud muſt be in the parts of the plain neareſt the ſides of the river, which in length of time will raiſe

theſe grounds above the reſt of the plain. Again, the waters in the bed itſelf depoſing inceſſantly a part of the mud, &c. brought down by the ſtream, muſt continually, though inſenſibly (for a long ſpace of time) raiſe the channel and banks of the river above the reſt of the plain. Theſe cauſes may at laſt contribute to the forming of an intire new bed for the river; for as all rivers carry down in their ſtreams more or leſs mud and other heterogeneous matters, which do not ſubſide regularly in all parts alike, but muſt precipitate faſteſt where the current is ſloweſt; there muſt accumulate by little and little in theſe parts ſuch banks of ſand and mud, as will in time hinder the current of the waters, make them reflow, and at laſt totally change their direction.

Canals are ſtill more ſubject than rivers to have their beds raiſed and their currents ſtopped by the ſubſiding of mud and heterogeneous matter in different places, and eſpecially juſt above their ſluices; becauſe of the ſudden ſtagnation of the water which firſt begins there as often as the ſluices are ſhut: and as there is a neceſſity for keeping them for the moſt part ſhut, the ſtagnating waters in their beds muſt precipitate their mud, &c. in a much greater proportion than can be done in the currents of rivers, which are in a continual motion towards the ſea.

42. I call *center of the current*, or, more properly, *line of greateſt current*, that *line which paſſes through all the ſections of a river, in the point where the velocity of the current is the greateſt of all.* We have ſeen above (N° 25.) that if the current of a river is regular, and in a right line, its center or line of greateſt velocity will be preciſely in the center of the ſections (N° 6.): but, on the contrary, if the bed is irregular and full of turns and windings, the center or line of greateſt current will likewiſe be irregular, and often change its diſtance and direction with regard to the centers of the ſections through which the waters flow, approaching ſucceſſively, and more or leſs, to all parts of the bed, but always in proportion and conformably to the irregularities in the bed itſelf.

This deviation of *the line of greateſt current* from the centers of the ſections through which it paſſes, is a cauſe of many and great changes in the beds of rivers, ſuch as the following:

1ſt, In a ſtraight and regular bed, the greateſt corroſion of the current will be in the middle of the bottom of the bed; becauſe it is that part which is neareſt to the line of greateſt current, and at the ſame time which is moſt acted upon by the perpendicular compreſſion of the water. In this caſe, whatever matters are carried off from the bottom will be thrown, by the force of the current,

current, equally towards the two ſides, where the velocity of the ſtream is the leaſt in the whole ſection.

2dly, If the bed is irregular and winding, the line of greateſt current will be thrown towards one ſide of the river, where its greateſt force will be exerted in proportion to the local cauſes which turn it aſide: in ſhort turns of a river there will be a gyration, or turning round of the ſtream, by reaſon of its beating againſt the outer ſide of the angle; this part will be corroded away, and the bottom near it excavated to a great depth. The matters, ſo carried off, will be thrown againſt the oppoſite bank of the river where the current is the leaſt, and produce a new ground, called an *alluvion*.

3dly, Inequalities at the bottom of a river retain and diminiſh the velocity of the water, and ſometimes may be ſo great as to make them reflow: all theſe effects contribute to the ſubſiding of ſand, earth, and other matters thereon, which ceaſe not to augment the volume of the obſtacles themſelves, and produce ſhallows and banks in the channel. Theſe in time, and by a continuance of the cauſes, may become iſlands, and ſo produce great and permanent changes and irregularities in the beds of rivers.

4thly, *The percuſſions of the center of the current* againſt the ſide of the bed are ſo much the greater as they are made

made under a greater angle of incidence; from whence it follows, that the force of percuſſion, and the quantity of corroſion and of detriment done to the banks and weirs of rivers, and to the walls of buildings made therein, and which are expoſed to that percuſſion, *are always in a direct compound proportion of the angle of incidence, of the greatneſs and depth of the ſection together, and of the quantity of velocity of the current.*

5thly, It may happen in time, that the excavation of the bottom, and the corroſion of the ſides, will have ſo changed the form of the bed as to bring the force of percuſſion into equilibrium with the velocity and direction of the current; in that caſe, all farther corroſion and excavation of the bed ceaſes (N° 31.)

6thly, This gives the reaſon why when one river falls into another almoſt in a perpendicular direction, and makes with it too great an angle of incidence, this direction is changed in time, by corroſions and alluvions, into an angle much more acute, till the whole comes into equilibrium.

7thly, So great and ſuch continued irregularities, from local cauſes, may happen in the motion of a river, as will intirely change its ancient bed, corrode through the banks, where they are expoſed to the greateſt violence of

percuſſion

percuſſion of the ſtream, and open new beds in grounds lower than what the old one is become.

8thly, Hereupon the ſtate of the old bed will entirely depend on the quantity of water, and on the velocity and direction of the current in the new one; for immediately after this diviſion of the waters into two beds is made, the velocity of the current in the old one will be diminiſhed in proportion to its leſs depth. In conſequence thereof, the waters therein will precipitate more of their mud, &c. in equal ſpaces than they did before; which will more and more raiſe up the bottom, ſometimes even till it becomes equal with the ſurface of the ſtream. In this caſe, all the water of the river will paſs into the new bed, and the old one will remain intirely dry. It is well known, that this has happened to the Rhine near Leyden, and to many other rivers.

9thly, Hence the cauſe of the formation of the new branches and mouth, whereby many great rivers diſcharge their waters into the ſea.

43. But in proportion as *a river, that has none of theſe obſtacles in its bed,* approaches towards its mouth, we ſee the velocity of its current augment, at the ſame time that the declivity of the bed diminiſhes, the cauſes of which have been explained above (N° 29.). It is for this reaſon, that inundations are more frequent and conſi-

derable, and do more damage in the interior parts of a country, than towards the mouths of moſt rivers.

In the Po, for example, the height of the banks made to keep in the waters diminiſhes as the river approaches to the ſea. At Ferrara they are twenty feet high; whereas nearer the ſea they do not exceed ten or twelve feet, although the channel of the river is not larger in one place than in the other.

44. The mouths of rivers, by which they diſcharge their waters into the ſea, are liable to great variations, which produce many changes in them.

1ſt, The velocity and direction of the current at theſe mouths are in a continual variation, cauſed by the tides, which alternately retard and accelerate the ſtream.

2dly, During the flowing of the tide, the current of the river is firſt ſtopped, then turned into a direction intirely contrary throughout a conſiderable extent; if we may believe M. DE BUFFON, there are rivers in which the effect of the tides is ſenſible at 150 or 200 leagues from the ſea.

3dly, This ſtate of things is a cauſe of a great quantity of ſand, mud, &c. being precipitated and accumulated in the channel near the mouth. This continually raiſes and widens the bed, and at laſt changes it intirely into a new place, or at leaſt opens new mouths to diſ-

charge the waters at. The Rhine, the Danube, the Wolga, the Indus, the Ganges, the Nile, the Miſſiſippi, and many other rivers, are inſtances of this.

4thly, All theſe effects are leſs ſenſible at the mouths of little rivers, as their currents oppoſe no ſenſible obſtacle to the flowing of the tides; ſo that the ebb carries off again what the flow had brought in.

45. Whenever the courſe of a river throughout a conderable extent of country approaches towards a right line, its current will have a very great rapidity; and the velocity wherewith it runs diminiſhing the effect of its natural gravitation, the middle of the current will riſe up, and the ſurface of the river will form a convex curve of ſufficient elevation to be perceived by the eye; the higheſt point of this curve is always directly above *the line of greateſt current* in the ſtream.

On the contrary, when rivers approach near enough to their mouths for a ſenſible effect to be produced in them by the flowing of the tides; and alſo when in other parts of their courſe they meet with obſtacles at the ſides of their channel: in both theſe caſes the ſurface of the water at the ſides of the current is higher than in the middle, even though the ſtream be rapid. In this ſituation of things, the ſurface of the river forms a concave curve, the loweſt point of which, or that of inflexion, is

directly over *the line of greateſt current.* The reaſon thereof is, that there are in this caſe two different and oppoſite currents in the river; that whereby the waters flow towards the ſea, and preſerve their motion therein even to a conſiderable diſtance; and that of the waters which remount, either by the flowing of the tide, or by their meeting with local obſtacles, which form a *counter current*, ſo much the more ſenſible as the flowing of the tide is ſtronger, or as the percuſſion of the water is made againſt greater obſtacles, and in a direction nearer to a perpendicular to them. From both theſe cauſes, the greater of which by far is that of the tides, the water near the ſides of the channel, where the velocity of the deſcending ſtream is naturally the leaſt (N° 25), takes a contrary direction, and runs back in the river, while that in the middle continues to flow on towards the ſea. This counter current is what the French call *a remous*.

An iſland in the middle of a river produces the ſame effect as obſtacles at the ſides, regard being had to the difference of ſituation of each.

Eddies and *whirlpools* in rivers, in the center of which there appears a conical or ſpiral cavity, and about which the water turns with great rapidity and ſucks in whatever approaches it, proceed in general from the mutual percuſſion of theſe two counter currents; and the vacuity in

the middle is produced by the action of the centrifugal force, whereby the water endeavours to recede, in a direct ratio of its velocity, from the center about which it moves.

46. If rivers perſevered always nearly in the ſame ſtate (N° 5.) the beſt means of diminiſhing the velocity of the current, when it is found too great for the purpoſes of navigation, would be by widening the canal: but as all rivers are ſubject to frequent increaſe and diminution, and conſequently to very different degrees of velocity and force in the current, this method is liable to produce very detrimental effects; for, when the waters are low, if the channel is very large in proportion, the ſtream will excavate a particular bed, which, according to the irregularities of the bottom, will form various turnings and windings with regard to the principal bed; and when the waters come to increaſe, they will follow, to a certain degree, the directions which the bottom waters take in this particular bed, and thereby will ſtrike againſt the ſides of the channel, ſo as to deſtroy the banks and cauſe great damages.

It would be poſſible to prevent in part the bad effects proceeding from the current ſtriking againſt the banks, by opening, at thoſe places where it ſtrikes, little gulfs into the land, dug in ſuch a form and direction as that the

ſtriking

ſtriking current ſhould enter and circulate therein, ſo as to deſtroy, or at leaſt greatly diminiſh, its velocity. This effect would be felt for a conſiderable way down the river.

This ſame method might probably be uſed with ſucceſs againſt the deſtruction of bridges, weirs, &c. by the violence of the ſtream during floods. Such gulfs being dug into the outer ſide of thoſe turnings in the river which are immediately above the place to be ſecured from the violence of the ſtream, would ſucceſſively diminiſh its velocity, its force and dangerous effects, a conſiderable way down the river. It is true, this method might contribute to produce an overflowing of the river upon the grounds adjacent to thoſe artificial gulfs, this being a natural conſequence of the decreaſe of the velocity of the current in thoſe places; and it would remain to be conſidered whether thoſe local inundations, or the danger of deſtruction of the bridges or edifices in the river, were the leſſer evil.

47. The nature of inundations, and the manner of their formation, merit a particular attention in this place.

While the volume of water in the *bed* of a river increaſes, the velocity of the current increaſes in proportion, as has been repeatedly ſhewn above (N° 13. 18. 20.

23. 27. 28. 29.). But from the moment that part of this water overflows the bed, the velocity thereof begins to diminiſh (N° 41.) and does ſo more and more, the farther it flows and ſpreads on the plain. So that the overflowing being once begun, it is a natural conſequence, that the inundation ſhould continue for ſeveral days; for though the volume of water brought down by the flood during that time ſhould decreaſe, yet, as the quantity of what runs off decreaſes likewiſe, from the great decreaſe of velocity in what overflows the plains, it will continue to produce the ſame effect as if the volume of water coming down had not diminiſhed, until the whole of the ſtream be every where contained again within the bed of the river. When that is become the caſe, the waters that have overflowed the plain will decreaſe thereon, by gradually and ſlowly running off, and alſo by evaporation, till they wholly diſappear. If this was not ſo, we ſhould ſee rivers overflow for an hour or two, and then return again within their beds, a thing contrary to general obſervation; for we conſtantly ſee inundations, once begun in flat countries, laſt for ſeveral days together, although in the mean while the rain ceaſes, and the quantity of water coming down diminiſhes. This muſt be the caſe, becauſe as the overflowing diminiſhes the velocity, and conſequently the quantity of water

carried

carried off, it has the ſame effect as if a greater quantity ſtill continued to come down.

It may not be uſeleſs to remark here, that what we have often ſaid in this eſſay becomes evident from theſe obſervations on nature, as well as from the principles laid down in it; to wit, that the moſt direct and efficacious method of preventing inundations is by deepening the bed and raiſing the banks of the river.

It may likewiſe be obſerved, with regard to inundations, that if the wind blows directly contrary to the current of the river, the overflowing will be greater than it would have been otherwiſe, becauſe this accident diminiſhes the velocity of the ſtream: but, on the contrary, if the winds blow in the ſame direction with the current of the river, the inundation will be leſs than otherwiſe, and ſooner at an end; becauſe this accidental cauſe augments the velocity of the ſtream.

VI. *On the confluence of rivers, and on the ſeparation of the ſame river into divers branches and mouths, with the effects thereof upon the velocity of currents, inundations*, &c.

48. All great and long rivers receive into their beds many others of different magnitude throughout the extent

tent of their courſe. This is evident to every one who only caſts his eyes over a map. The Rhine and the Po, in particular, receive each above one hundred others great and ſmall; the Danube above two hundred; the Wolga as many; the river of Amazons receives into its vaſt bed a prodigious number, ſome of which are five or ſix hundred leagues in length, and are of ſuch a depth and breadth as would make them elſewhere paſs for capital rivers. M. DE BUFFON (*) gives a liſt of the more conſiderable of thoſe which fall into other great rivers throughout the known world. Many curious particulars may be ſeen in VARENIUS's General Geography, part I. chap. XVI. concerning rivers; but of a nature which does not enter into my plan. The works themſelves are in every body's hands, and may be conſulted by thoſe who pleaſe.

This confluence of rivers is ſo neceſſary for propagating the motion of the water throughout a long courſe, and for renewing and accelerating from time to time its velocity, which otherwiſe would be too greatly diminiſhed by the reſiſtance of ſo many obſtacles as they meet with in their way, that, as we have ſaid above (N° 28.) after Signor GUGLIELMINI, it can only be attributed

(*) Hiſt. Nat. tom. II. p. 75, 76.

to

to the infinite wiſdom of the Author of Nature, in the original diſpoſition of things.

49. We have ſeen above (N° 18. 27. 28.) that the increaſe of a river or canal by the new waters which it receives, *is univerſally in an inverſe ratio of the new velocity which is acquired therefrom.* If this velocity is greater, the increaſe of the ſection of the new ſtream will be leſs in proportion, and *vice verſâ.* It follows from hence, that it is poſſible for one river or open canal to fall into another river or open canal of equal magnitude with itſelf, and yet the ſection of the current in the common bed after their confluence ſhall be no greater than it was in each of them before their junction. It is certain that this will be the caſe as often as *the confluence of the two augments the velocity of the common current in the ſame proportion with the increaſe of the volume of waters*; both the greater rapidity of the current, and the greater volume of water in the bed after the junction, ſerving to deepen it in proportion to its breadth, will contribute towards the above effect. Another cauſe will likewiſe add thereto; to wit, that inſtead of the reſiſtance from the attraction, friction, and other obſtacles, in two beds, which give two bottoms and four ſides, there are, after the confluence, only thoſe of one bed, of one bottom and two ſides. Moreover, the center of the ſection in the common bed

is farther from the bottom and ſides thereof, than it is in the ſeparate beds. All theſe cauſes, in proportion to their reſpective quantities, contribute to accelerate the velocity of the common ſtream.

50. It is not leſs certain, that in rivers which bring down a great abundance of water, the more the velocity and diſcharge thereof at their mouths are retarded and diminiſhed by the tides, the winds, the rolling in of the ſea, &c. the more will the back-waters increaſe in height, and endanger overflowing the inner parts of the country. This is evident, becauſe the decreaſe of velocity in the current, and the increaſe of height of all the back-waters that are affected thereby, are in a reciprocal inverſe ratio one of another (ſee above N° 32.).

Nature itſelf teaches us a method of preventing, or at leaſt of diminiſhing, this effect. We ſee all great rivers overcharged with a vaſt volume of water divide, when they come near the ſea, into different branches and mouths, whereby the ſuper-abundance of their waters is diſcharged. This is the caſe with the Scheld, the Rhine, the Rhone, the Po, the Danube, the Wolga, the Euphrates, the Indus, the Ganges, the Nile, the Niger, the Oroonoko, the River of Amazons, and with almoſt all other great rivers.

This ſeparation and diſperſion of the too great quantity of water into ſeveral channels is one cauſe of their ſeldom overflowing the country near their mouths (*f*), becauſe it gives a greater depreſſion and declivity to the ſurface of the current, and thereby facilitates the running down of the waters from the interior parts of the country, foraſmuch as their beds are every where regular and free from obſtacles to their current.

51. Notwithſtanding the apparent oppoſition to what has been ſaid in ſeveral other parts of this treatiſe, I repeat again, that this diviſion and diſperſion of the waters into ſeveral branches and channels *when there is ſuch an abundance of it as is ſufficient to keep up the velocity both in the old and new channels*, augments the declivity, and thereby facilitates the running off of all the back-waters from the inner parts of the country, as far as the bed is regular and free from obſtacles, according to what is laid down above (N° 38.).

But whenever this ſuper-abundance of waters, ſufficient for keeping up the velocity in each channel nearly to what it was before the ſeparation or divarication, ſhall be found wanting, it is certain, that this diviſion and diſperſion of the waters into ſeveral channels will only ſerve to diminiſh the velocity of the current in each, whereby

(*f*) Another cauſe thereof is pointed out above, N° 43.

as much or more diſcharge of the water, and conſequently of declivity for the running off of the back-waters, may be loſt, as has been gained by the ſeparation into different beds.

This diſadvantage may be eaſily remedied in thoſe new channels and mouths of rivers which are dug by hands, and have ſluices placed in them at the point of ſeparation from the original bed; for theſe ſluices of communication need be opened only when there is a ſuper-abundance of water in the river, ſufficient to keep up the velocity in each of the channels; at other times they may be kept ſhut, and the waters retained in their original bed.

52. It was for this purpoſe, of preventing the damages proceeding from immoderate inundations, that the ancient Egyptians dug vaſt lakes, and made ſo many canals and ſluices of communication between the Nile and thoſe lakes, and from thence to the ſea; that they might thereby be able to diſcharge the waters into thoſe reſervoirs if they came down in too great abundance, or let them off again from thence upon the land, if the quantity of the natural inundation at any time was leſs than what was neceſſary for the good of the country. By theſe means ancient Egypt was always maſter of its waters.

It is well known that it rains ſeldom in that country, and that the Nile by its regular inundations waters the land, by bringing down upon it the rains and melted ſnow from the high mountains of Abyſſinia. HERODOTUS (*g*) and DIODORUS SICULUS (*h*) have left us deſcriptions of the immenſe labours of the inhabitants to govern and multiply ſo beneficent a river, the particulars whereof are too well known to be repeated in this place. By theſe means Egypt became the granary of the world for above two thouſand years, and reimburſed, with immenſe advantage, the firſt expences.

RICCIOLI (*i*) aſſures us, that the ancient Perſians did the ſame thing with regard to the *Euphrates*, and for the ſame end. He adds, moreover, *Sic ubi Cyrus Gangem in Alveos* 460 *diſperſit, minora damna ex Gangis alluvionibus campi perpeſſi ſunt*; but I am totally at a loſs to find upon what authority he grounds this laſt aſſertion, for I never read that any CYRUS penetrated as far into India as the mouth of the *Ganges*, much leſs reigned ſo long over that country as to perform the vaſt work which RICCIOLI ſpeaks of.

(*g*) In lib. II.
(*h*) Biblioth. l. II. c. 1.
(*i*) Geogr. et Hydrogr. l. VI. cap. XXIX. p. 248, 249.

PLINY

PLINY (*k*) ſays, with regard to the different mouths of the Po, *Omnia ea flumina foſſaſque primi à Sagi fecere Thuſci, egeſto amnis impetu per tranſverſum in Atrianorum Paludes, quæ Septem Maria appellantur.* Theſe ſeven lakes diſcharged their waters into the ſea by ſeven mouths, which PLINY names in the ſame place. All this was apparently done that the river might do leſs damage to the adjacent countries by its frequent inundations. PLINY adds, *His ſe Padus miſcet, ac per hæc effunditur, pleriſque, ut in Ægypto Nilus, quod vocant Delta.*

To theſe examples, drawn from ancient hiſtory, might be added many modern ones, if the things in queſtion had need of further proofs. Thus, both nature and the experience of a long ſeries of ages teach us, that the ſeparation of a river into ſeveral beds, by new branches and mouths, is a means of diminiſhing inundations in the inner part of the country; but that this takes place only when there is a ſufficient abundance of water in the river to fill the new beds and channels ſo far as to prevent the velocity of the currents therein from being notably diminiſhed from what they were before the diviſion.

(*k*) Hiſt. Nat. l. III. cap. XVI.

SEC-

SECTION II.

Laws of the meeting of oppoſite currents, with the application of them to ſluices.

53. When two equal currents of homogeneous fluids meet in oppoſite directions, there is firſt a ſwelling and riſing up of them at the point of rencounter; then follows a revulſion and counter current of each equally back again, ſo as to bring the whole to an equilibrium.

54. If the two oppoſite currepts are unequal, either in force or in quantity, or in both, there will ſtill be a ſwelling and revulſion of each back again, but it will be diminiſhed in the greater current, and augmented in the leſſer, by the quantity by the which the one ſurpaſſes the other; and the point of rencounter of the two currents will have a ſlow and progreſſive motion in the direction of the ſtronger, the degree of velocity thereof being always in a direct ratio of the force and quantity of the one above the other,

55. If the fluids in oppoſite currents be not homogeneous, as is the caſe between ſea and river water, that which has the leaſt ſpecific gravity will ſwim upon the other, and continue to follow its firſt direction, until ſuch

ſuch time as the heavier fluid ſhall have communicated its motion to all the parts of the lighter. But the lighter fluid will not loſe its former motion and direction at once, but *in a decreaſing ſeries, the law whereof will vary according to the greater or leſs difference of ſpecific gravity in the two fluids*, until the whole of the lighter has acquired the velocity and direction of the heavier which buoys it up.

The *time* and *ſpace* required for a greater current of ſalt water to communicate its motion and direction to an oppoſite one of freſh water will be but very little, ſince they differ in ſpecific gravity only $\frac{3}{71}$ parts that the ſalt is heavier than the freſh. It would require much greater between water and oil, and ſtill much more between quickſilver and oil, and ſo on. The elements for determining them in every caſe might be found by a proper number of experiments.

56. Let the two currents be equal or unequal in force and velocity *but nearly of the ſame ſpecific gravity*, if we ſhould ſuppoſe at the ſame time that their ſurfaces are not upon a level, but that the one is higher than the other (as is conſtantly the caſe in all ſluices that open to the ſea, except at the moment when the ſurface of the tide is upon a level with the ſurface of the water in the canal behind the ſluice); this circumſtance entirely changes both the caſe and the effects. It is certain, on this

this ſuppoſition, that the overplus of velocity and elevation in the higher current, though it ſhould be the leſſer, will make the waters in the lower and greater current reflow upon themſelves until they come to a level and equilibrium with thoſe in the upper current; ſince theſe, by the laws of univerſal gravitation, cannot flow back from a lower to a higher level, but muſt deſcend according to the declivity of the ſurfaces. If the currents are of *very different ſpecific gravities*, they will come to an equilibrium according to the law laid down above (N° 55); but their greater or leſſer quantity and velocity will produce little or no effect in this caſe.

57. Now as the running of two currents in oppoſite directions, after their rencounter, and beyond the limits laid down above (N° 55.), is incompatible with, and contradictory to, the laws of nature, and conſequently impoſſible; we may draw this uſeful concluſion, which becomes important during inundations, and eſpecially during the annual overflowing of the low grounds in flat countries; to wit, that if the ſluices next the ſea againſt which the tide flows be ſhut only a quarter of an hour before the *flood* has riſen to the level of the water in the canal, not a drop of ſalt water can enter the ſaid canal, nor even into the ſluice itſelf; becauſe both the progreſſive motion of *the point of rencounter of*

the two currents, and the *over-ſwimming* of the freſh water upon the ſalt, will be always without the ſluice and towards the ſea, ſo long as the ſurface of the tide is below the level of the water in the canal. Many ſluice maſters, for want of knowing or conſidering this, are accuſtomed to ſhut their gates next the ſea a little after *half flood*, under the pretence of preventing by this means the ſalt water from getting into the canal, and communicating thereby with the waters that overflow the low grounds in many places during winter, which would be of great detriment to the ſoil. Through this falſe perſuaſion, they loſe no inconſiderable part of that time every day, which they might ſafely employ in drawing off the waters which overflow and incommode low and flat countries almoſt every winter and rainy ſeaſon, as is the caſe in the Dutch and Auſtrian Netherlands.

SECTION IV.

Experiments to determine the different velocities, in different depths of water, of the ſame floating body moved uniformly by an equal force.

58. It is well known already, that for facilitating or retarding the motion of boats, &c. in canals, the different

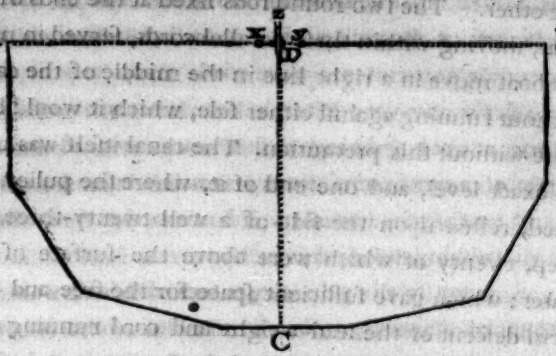
depths of the water, above that ſimply neceſſary to keep them afloat, is a thing not at all indifferent. Dr. FRANKLIN has already treated this ſubject, though perhaps not with ſufficient accuracy, in a letter to Sir JOHN PRINGLE, written in the year 1769. He proves, however, that it is univerſally known among people accuſtomed to work boats on canals, that there is a conſiderable difference in the ſwiftneſs of their motion according to the greater or leſs depth of the water therein; and that the water being low is of itſelf ſufficient to retard the motion of a boat, without the keel thereof rubbing againſt the bottom of the canal. The reaſon he aſſigns for it is evident; for a boat cannot advance its own length in a canal without diſplacing a quantity of water equal in maſs to the ſpace which the boat occupies under the ſurface of the fluid. The water ſo diſplaced muſt retrograde, and paſs under, and to the right and left, of the boat: ſo that the leſs depth and breadth of water there is in the channel, the more in proportion it muſt riſe up and weigh againſt the boat, and the more difficulty it muſt find in paſſing under and along ſide of it, and neceſſarily muſt retard ſo much the more the motion thereof. The reſult of Dr. FRANKLIN's experiments on this ſubject may be ſeen in the letter above mentioned.

59. Mr. NEEDHAM, Director of the Imperial Academy of Sciences at Bruxelles, being of opinion that Dr. FRANKLIN's experiments were made upon too ſmall a ſcale to draw any very exact inferences from them, deſired me, at the beginning of the year 1775, to make a new ſet of experiments upon a much larger ſcale and with all poſſible exactneſs; I did accordingly, and ſhall here give a ſhort deſcription of them.

I got made, by the ſhip carpenters of Nieuport upon the Coaſt of Flanders, an exact model of a bilander, anſwerable in all its proportions to thoſe uſed in the Low Countries. Its length was thirty-nine Engliſh inches, its breadth nine inches and a half, and its depth nine inches. Its form both within and without exactly repreſented that of a bilander. At each end of it was faſtened perpendicularly a round and poliſhed rod, ten inches and and a half in height above the ſides of the boat.

I got made likewiſe a wooden canal, twenty feet in length, thirty-ſeven inches in breadth, and ſixteen inches in depth; a ſection of which is repreſented in the following figure:

This



This form is that of the excavation of the canals in the Low Countries, and approaches to that of the natural beds of rivers inaſmuch as they are regular. Here AB = 37 Engliſh inches, DC = 16 inches; and the length of the whole canal, as we ſaid before, was twenty feet. z repreſents the ſection of a pulley fixed at one end of the canal, upon which paſſed a ſmall cord, one end of which was tied to the round rod at the fore part of the boat, and at the other end was a piece of lead which weighed eight ounces. This ſerved for an equable force to give an uniform motion to the boat throughout all the experiments. *x* and *y* are ſections of two other cords ſtretched parallel to each other at about one inch and a half diſtance, and reaching from one end of the canal to

the other. The two round rods fixed at the ends of the boat, moving within theſe parallel cords, ſerved to make the boat move in a right line in the middle of the canal, without running againſt either ſide, which it would have done without this precaution. The canal itſelf was upon an exact level, and one end of it, where the pulley was fixed, reſted upon the ſide of a well twenty-three feet deep, twenty of which were above the ſurface of the water; which gave ſufficient ſpace for the free and uniform deſcent of the lead-weight and cord running over the pulley, as they drew the boat from one end of the canal to the other.

Latts, exactly divided into inches, were nailed againſt each end of the canal within, to mark the different depths of the water in it according as it ſhould be augmented or diminiſhed. The outſides of the little boat, from its keel upwards, were likewiſe divided into inches. In the inſide of the boat was a quantity of ſand ſufficient to ſink it to ſix inches deep in the water. The common loaded bilanders in the Low Countries uſually draw ſix feet of water.

Thus the form of the wooden canal, together with its breadth and depth, and the form and dimenſions of the little boat, together with the depth of water it drew by means of its ballaſt of ſand, exactly correſponded with

thoſe

thoſe in the real canals and bilanders in the Low Countries, an inch in the one anſwering to a foot in the other.

Cloſe to the canal, and out of the way of all wind, was ſuſpended a pendulum of fine waxed thread, to prevent the variations of the atmoſphere from altering its length, which from the point of ſuſpenſion to the center of gravity in the lead was $39\frac{1}{5}$ Engliſh inches, ſo that its iſochronic vibrations were exactly ſeconds of time.

60. It was neceſſary, in order to render the experiments exact, that they ſhould be made at a time when the air was perfectly calm; for the leaſt breath of wind, during the motion of the boat, cauſed great variations and irregularities in them, which it was abſolutely neceſſary to prevent, in order to be able to deduce any exact reſults from them. On the contrary, in a perfect calm, the times of the paſſage of the boat, from one end of the canal to the other, were exceedingly regular, as may be ſeen from the table of experiments which I give below.

By means of the pendulum I was able to meaſure the times of paſſage of the boat along the canal, in all the different depths of water, to a third or even to one quarter of a ſecond. The boat being held faſt againſt the back end of the canal by the hand of an aſſiſtant, and then let go, it was eaſy for me to perceive the preciſe inſtant

inſtant of the beginning of its motion, to let go the pendulum at the ſame moment, and to count its vibrations till the inſtant that the boat ſtruck with an accelerated force againſt the fore end of the canal. As to the weight of eight ounces ſuſpended at the end of the cord, and which ſerved as *a moving force* to draw the boat along the canal, it was juſt as much as ſufficed to counterbalance the cord, and to put the boat in motion; leſs weight than that would do neither: therefore I was obliged to uſe ſo much, notwithſtanding the conſiderably accelerated motion it gave to the boat.

This is the whole mechaniſm of the inſtruments I uſed for the experiments in queſtion, and ſuch were the precautions I judged it neceſſary to uſe for making them with ſcrupulous exactneſs.

In the following table, which conſiſts of twelve columns, the firſt of them contains the different depths of water at which the experiments were made; the ten following ones contain ten different experiments made at each depth of water in the canal; and the twelfth or laſt column is the reduction of the ten others to a mean proportional or mean reſult of the whole, which is in *ſeconds of time*.

Table

Table of experiments made to aſcertain the times of paſſage of the boat along the canal, or its different degrees of velocity, in different depths of water.

Depths of Water.	1ſt Exp.	2d ditto	3d ditto	4th ditto	5th ditto	6th ditto	7th ditto	8th ditto	9th ditto	10th ditto	Mean Reſults.
Inches	"	"	"	"	"	"	"	"	"	"	"
15	14½	14¾	14⅗	14⅗	14½	14⅗	14⅗	15	14½	14⅗	14½
14	14¾	15	15	15½	15	15	14¾	14½	15	15½	15
13	15½	15½	15½	15½	15½	15¾	15¾	15½	15¾	15¼	15½
12	16½	16	16	15½	16	15¾	16	16	16	16	16
11	16¾	17	17	16½	16¾	17	16¾	16¾	16¾	16¾	16¾
10	17	18 wind	17	17½	17	17½	17½	17	17	17¾	17½
9	18½	18½	18	19	19	18	18	18½	18	18⅓	18⅓
8	20	20	20¼	20	20	20	20	19⅝	20	20	20
7	23½	23	22½	23	23	23	23½	23¾	23	23	23
6½	In the experiments made with this depth of water, the boat often touched the bottom.										30 by ſuppoſition

It may be obſerved with regard to the laſt column of the above table, which contains the mean reſults or mean quantities of time which the boat takes to paſs from one end of the canal to the other in different depths of water, that it is given for the ſake of deſtroying thoſe little differences which are inevitable in practice; and it

ſhews, as nearly as poſſible, what the true time of paſſage ought regularly to be when nothing happens to diſturb it.

It is alſo highly worthy of remark, that the *mean reſults* contained in this laſt column form a *ſeries of numbers regularly increaſing as the depths of water, wherein the reſpective experiments were made, regularly decreaſe*; ſo that *the different velocities of the floating body are in an inverſe ratio of the reſpective depths of the water in which it floats with an equal impulſive force, and that according to the law of the above ſeries.* This, perhaps, may furniſh elements to calculate, pretty near the truth, the different velocities of veſſels upon canals and rivers with different depths of water in all other caſes whatſoever. As to the concluſions to be drawn therefrom in practice, and in the common uſes of life, they are too obvious to need mentioning here.

SECTION V.

On the quantitity of declivity in rivers.

62. Abſtracting from all reſiſtance and friction, fluids, ſuch as water, deſcend upon planes let them be never ſo little inclined towards the center of the earth: and the

velocity of deſcent increaſes in a compound ratio of the increaſe of the maſs of water, and of the greater declivity of the plane which ſerves for its bed (N° 13.).

63. Water, though unaffected by any compreſſion or impulſion from above, cannot remain immoveable in any bed whatever except that which makes a curve perfectly concentrical with the *terreſtrial curve*; but in this, being every where equally affected by the force of gravitation, it will remain without motion any way.

64. It follows from hence, that the ſprings and ſources of all rivers muſt be at a greater diſtance from the center of the earth than one ſemi-diameter thereof, which is terminated at the ſurface of the ſea; without which the waters could not run to the river's mouth.

65. Therefore, the abſolute elevation of the ſurface of rivers is continually diminiſhed as they recede from their ſprings, becauſe of the neceſſary declivity of the beds of rivers towards the center of the earth; for without ſome degree of this declivity the waters could not run at all, as has been ſaid above (N° 62. 63.).

66. The declivity of the beds of rivers cannot be a right line making a rectilinear angle with that horizontal plane which, being continued, would interſect their reſpective ſources; but, if it is regular, it muſt be a curve which differs very little from that of the earth's ſurface,

and this, if the direction is in the parallels of latitude due Eaſt and Weſt, is *ſpherical*; but in all other directions it is a portion of an *oblate ellipſis*, on account of the earth's being a ſpheroid compreſſed by its axis. Now the horizontal plane which continued paſſes through the ſprings of rivers, is always a tangent to the curves of their beds at the point of inflexion, inſomuch as theſe are regular.

67. The quantity of abſolute declivity from the ſpring in any determinate part of a river, is that perpendicular line drawn from the point of greateſt current in that place till it meets *the curve concentrical to the earth's ſurface* which paſſes through the river's ſource. The declivity of the bed below the ſpring is had by taking the ſame perpendicular from the bottom of the bed; as that of the river's ſurface is by taking the perpendicular from thence.

68. If a plane be extended horizontally every way from the point of tangency to the earth's ſurface, or from the point where it is perpendicular to any radius of the earth, water will run from every other part of the plane towards that ſaid point which is nearer to the center of the earth than any other point in the whole plane.

69. The depreſſion of *the curve of a river's bed*, below *the concentrical-terreſtrial-curve which interſects its ſource*,

ſource, being only 250 fathom perpendicular in a courſe of 500 leagues, it will be ſufficient to give a notable current in a regular bed throughout all that extent of river, as appears from what we have ſaid above (N° 38.). But the depreſſion of *the curve of the river's bed*, below *the horizontal plane which is a tangent to its ſource*, in this ſame extent of courſe, is not leſs than ninety leagues perpendicular, being always *the ſecant of the arc of the river's extent minus a radius of the earth in that point.*

70. It follows evidently from the above principles (N° 62—69), that the declivity and velocity of a river are leſs in proportion as the bed approaches nearer to being concentrical with the curve of the earth's ſurface.

71. I ſhall now apply the principles laid down to determine, as near as poſſible, the real quantity of declivity in different rivers, making uſe of what is already known from experiments and actual menſuration to determine the ſame in all others by the compariſon of the different degrees of velocity in their reſpective currents.

72. It is the general opinion of moſt of thoſe who have examined this ſubject (1), that rivers and canals which have leſs than one foot of declivity in 10,000 feet of courſe, will have very little current, unleſs it be by means of the great abundance of their upper waters

(1) Vide DESCHALES, de Font. et Flow.

which give motion to those before them by their weight and impulsion. Without this the resistance proceeding from the bottom and sides of the bed, and from other accidental obstacles (N° 21.) would equal, if not surpass, the ordinary causes of acceleration (N° 20) so as to diminish continually the motion of the waters, and at last render them almost stagnant (N° 22.). But nature has prepared remedies against this, as we have seen above (N° 28. 29. 48.). What RICCIOLI (l) says of the *Po*, in that part of its course next its mouth, is perfectly conformable to this theory: "Sic Padus, qui a Pago *Ostellata* "vocato, usque ad *Adriaticum*, intervallo milliarium cir- "citer 70, non habet libramentum majus 13 aut 14 "pedum, ita ut singulis milliaribus ne 3 quidem unciæ "declivitatis obveniant; unde *Paduæ*, potius instar stag- "nantibus aquis, incertissimus esset ad defluxum cursus: "impetu tamen impresso à 30 et amplius fluminibus "aut torrentibus se in illum exonerantibus, etiamque à "nativæ pondere aquæ ex superioribus et altioribus "prope Alpes alveis decurrentis, velocitatem maximam "acquirit."

73. From many observations and trials which I made for this purpose in the years 1773 and 1774 upon the river Iprelee in Flanders, which comes down from the

(l) Geogr. et Hydrogr. l. VI. c. XIX. p. 215. edit. Bonon. 1661.

city

city of Ipres and falls into the ſea at Nieuport, having *a very moderate current* when the ſluices upon it are open, I found its *mean declivity* to be nearly three fathoms four feet and eight inches in 20,000 fathoms of extent of its courſe, or very nearly one foot in a meaſured Engliſh mile. I ſay its *mean declivity*, becauſe from what has been ſaid above (N° 13. 27. 28. 29.) it is plain, that a greater or leſs quantity than ordinary of water in it will add to, or take from, ſomething thereof; but the declivity in each part of its bed is nearly uniform.

As the ſources of this river, and thoſe of the Iſere which joins it at Fort Knock, four leagues from Nieuport, are in the higher grounds of Flanders towards Houthem, Mount Kemele, Swaertſberg, Catſberg, and the other hills as far as Mount Caſſel; and as the reſt of their courſe is in a flat country with a very ſmall deſcent towards the ſea, the declivity thereof may be taken as *a mean* between that of the other rivers and canals of Flanders: the *artificial canals* will have leſs, not above a ſix or ſeven thouſandth part of their extent, or one twelfth of an inch in each eight fathoms: the rivers Lys and Eſcaut, before they fall into the flat country, ſomething more, after which they may have about the ſame, or even ſomething leſs between Ghent and Antwerp.

This

This quantity of three fathoms four feet and eight inches of declivity in 20,000 fathoms of extent, gives the proportion of the declivity to the extent as 1 to 5292, which is one line or twelfth part of an inch in $6\frac{1}{8}$ fathoms, and two feet ſeven inches in one French league of 2283 fathoms. Now the meaſured Engliſh mile containing 5280 feet, this proportion of $\frac{1}{5292}$ approaches ſo very near to one foot of declivity in every meaſured mile of extent, that I ſhall every where reduce what I call the *mean declivity* to that quantity, as a ſtandard wherewith to compare the reſt.

74. In canals, all whoſe ſluices and vents have been kept ſhut a ſufficient time to render the water ſtagnant throughout their whole length, there cannot be allowed above an inch or two of declivity for each mile in length, on account of the water that unavoidably runs off through the chinks of the doors of ſluices, drains, &c.

75. According to the obſervations of the Abbé CHAPPE D'AUTEROCHE [m], the floor of the Hall of the Royal Obſervatory at Paris is forty-five fathoms three feet and five inches French above the level of the ſea at the mouth of the Seine. According to the Abbé NOLLET, this ſame floor is forty-ſix fathom above the level of the Ocean, and only forty-five fathoms above the level of the Mediter-

(m) See Relation de ſon Voyage en Siberie, tom. II. p. 406, 407. 444.

ranean

ranean ſea. Again, according to the above Abbé CHAPPE, the ſaid floor of the Obſervatory is elevated twenty-four fathoms one foot and ten inches above the level of the river Seine at Paris; therefore the level of the Seine under the Pont Royal at Paris is twenty-one fathoms one foot and ſeven inches above the level of the Ocean; and ſuch alſo is the quantity which Meſſ. CASSINI have given, from their own obſervations and experiments, for the mean height of the Seine at Paris above the level of the ſea.

Now the courſe of the Seine from Paris to its mouth at Havre de Grace, by following all its turns and windings, is about 90,000 fathoms in length; therefore $\frac{90,000}{21.1.7} = 4232\frac{1}{2}$ fathoms of extent for one fathom of declivity in the river Seine, or one line in $4\frac{1}{3}$ fathoms, and conſequently the proportion of its declivity to its length is as one to $4232\frac{1}{2}$. It is to be obſerved, that the bed of the Seine is deep, and its current conſiderably ſtrong.

76. By ſimilar obſervations and actual levellings made upon the river Loire by M. M. PICARD and PITOT [n], the declivity thereof in proportion to its length is found to be as one to 3174, which is one line in $3\frac{1}{3}$ fathoms. Notwithſtanding this great declivity of the bed of the Loire it is obſerved, that the velocity of the water therein,

(n) See Memoires de l'Acad. Royale des Sciences de Paris, pour 1730.

compared to that in the Seine, is leſs than it ſhould be in proportion to their reſpective declivities, which is very juſtly attributed to the much greater depth in proportion to the breadth of the Seine, above what is found in the Loire. This laſt river is remarkably broad, and ſo ſhallow that in many places it is hardly navigable for boats. Now this, according to the principles laid down above (N° 27, &c.) muſt very much diminiſh the ſwiftneſs of the current, which it ſhould otherwiſe have from the great declivity of its bed. In confirmation of this it is moreover obſerved, that in great falls of rain, which equally increaſe the volume of water in both theſe rivers, the velocity in the Loire augments in a much greater proportion than it does in the Seine; and this obſervation is likewiſe conformable to the principles above laid down (N° 12. 28. &c.).

77. The river Doux, after paſſing by Beſançon, falls into the Saone above Chalon; the Saone joins the Rhône at Lyons. This river, from Beſançon to its mouth in the Mediterranean ſea, is one of the moſt rapid in the known world: the velocity of its current is at leaſt double to that of the Seine or Loire, and its courſe is almoſt in a ſtraight line. The difference of elevation of this river at Beſançon, above that of its mouth in the Mediterranean ſea, after a courſe of about eighty-ſix French leagues, has been

been found, by a long ſeries of barometrical obſervations, to be about ſeventy-five fathom (*s*), which gives the proportion of the declivity to the extent as one to 2620, or about one third of a line to each fathom. This is double the *mean declivity* of the rivers in Flanders; but the velocity of the current in the Rhone is at leaſt triple that in the others (N° 29.).

78. From the above *data*, got from obſervations and actual menſuration, and from many others of the ſame nature too long to mention here, we may deduce the following table of comparative proportions between the declivities and velocities in different kinds of rivers.

(*s*) See Cours de Phyſique de Para, tom. II. N° 740.

Rates or claſſes of rivers and flowing waters.	Comparative degrees of the mean velocities of currents	Seconds of time wherein currents run 20 fathom.	Fathom run by the current in 1 minute of time.	Ratio of declivity compared with horizontal length.	Fathoms of length for each ¼ inch of declivity.	Diſtinctive attributes of the various kinds of rivers and flowing waters.
1	0	" 0	0	1.0000	14	Channels wherein the reſiſtance from the bed, and other obſtacles, equal the quantity of current acquired from the declivity; ſo that the waters would ſtagnate therein, were it not for the compreſſion and impulſion of the upper and back-waters.
2	⅔	180	6⅔	7000	8	Artificial canals in the Dutch and Auſtrian Netherlands.
3	1	120	10	3000	6	Rivers in low and flat countries, full of turns and windings, and of a very ſlow current, ſubject to frequent and laſting inundations.
4	1½	80	15	1000	4⅔	Rivers in moſt countries that are a mean between flat and hilly, which have a good current, but are ſubject to overflow: Alſo, the upper parts of rivers in flat countries.
5	2½	55	21⅔	700	3⅔	Rivers in hilly countries, with a ſtrong current, and ſeldom ſubject to inundations: Alſo, all rivers near their ſources have this declivity and velocity, and often much more.
6	3	40	30	1800	3	Rivers in mountainous countries, having a rapid current and ſtraight courſe, and very rarely overflowing.
7	5	24	50	1000	2⅓	Rivers in their deſcent from among mountains down into the plains below, in which places they run torrent-wiſe.
8	8	15	80	1700	2	Abſolute torrents among mountains.

I ſhould

I ſhould think it quite ſuperfluous to give any explanation of a table ſo clear and intelligible as the above; and ſhall only remark upon it that *the comparative degrees of the mean velocities of the reſpective currents* in the ſecond column are the reſult of obſervations and experiments, the method of making which has been given above (N° 26.): but as the velocity of rivers is very different in different ſeaſons of the year, which augment or diminiſh greatly the maſs of waters in their beds, *a mean* has been kept to, as much as poſſible, in the above table.

By taking the degree of velocity of the current in any river, a thing ſo eaſy to be done; and obſerving its other characteriſtics as laid down above under the title of *diſtinctive attributes*, it will be eaſy to judge very nearly of *the quantity of declivity in that part of the river.*

79. After carefully comparing what has been ſaid in the relations of travellers, and in the beſt treatiſes of geography, upon the principal rivers in the known world, I ſhould be inclined to claſs them in the following manner, particularly entreating at the ſame time that my opinion about it may be regarded as ſimple conjecture, which I leave to be rectified by thoſe better acquainted with the matter than it is poſſible for me to be.

Under the firſt rate or claſs in the above table I ſhould put that part of the channel of moſt great rivers which

is

is in extenſive plains next the ſea; with regard to *the declivity thereof alone*, but not at all with regard to the velocity of the current there, which is often very great from the compreſſion and impulſion of the upper waters, as has been repeatedly ſhewn above muſt be the caſe (N° 29. 38. 43. 72.).

Second rate or claſs. Moſt artificial canals in flat countries, made for the uſe of navigation; eſpecially thoſe in the Dutch and Auſtrian Netherlands.

Third rate or claſs. The river Trent; the Scheld and the Lys below Ghent; the Iſere and the Iprelee below Fort Knock in Flanders; many rivers in the territories of Bologna and Ferrara in Italy; the river Meander in Natolia; and innumerable others in flat countries.

Fourth claſs. The Thames; the Lys and the Scheld above Ghent in Flanders; the Senne, the Dyle, and the Demmer, in Brabant; the Seine and the Somme in France; the Nile and the Niger in Africa; the rivers of St. Lawrence below Lake Ontario, the Oroonoko, the river of Amazons, and the rivers of Paraguay, in America.

Fifth claſs. The Severn and Ouſe in England; the Loire and Garonne in France; the Tagus, the Guadiana, and the Guadalquivir, in Spain; the Po and the Tiber in Italy; the Meuſe, the Rhine, and the Elbe, in Germany; the Weiſſel, the Neiſter, the Bog, and the Nieper,

in

in Poland; the Don and the Dwina in Ruſſia; the Amur or Saghalien in Tartary; the Yellow and Blue Rivers in China; the rivers of Cambodia, Ava, and Ganges, in India; the Euphrates; the river Zaire in Congo; the Miſſiſippi.

Sixth claſs. The Rhone in France; the Ebro and Douro in Spain; the Danube; the Wolga; the Irtiſch and Oby, the Jeneſea and Lena, in Siberia; the river Indus; the Tigris; the Malmiſtra in Cilicia.

Seventh claſs. In this claſs can only be enumerated thoſe parts of rivers where they deſcend from among mountains into the plain country below; as alſo ſome rivers paſſing through the midſt of mountains.

Eighth claſs. To this claſs belong all torrents among mountains; ſuch, for example, as the Bourns in the Highlands of Scotland are deſcribed to be.

SECTION VI.

A general and eaſy method of taking levels through large extents of country where rivers paſs; and alſo of computing the heights of interior parts of continents above the ſurface of the ſea.

80. After all I have ſaid hitherto in this eſſay, and particularly in the foregoing ſection, what I am about to lay

lay down under this laſt head of it muſt appear very plain and eaſy. I am very far, however, from giving the methods I am going to propoſe *for taking the levels through whole countries and continents as far as rivers extend*, as ſtrictly exact; I know very well that it is next to impoſſible they ſhould be ſo, conſidering the continual variations in the declivities of rivers, and in the velocities of their currents in different parts, as alſo the impoſſibility of knowing the exact length of their courſe through all their turns and windings I only give them therefore as a general and eaſy method of computing the relative heights of countries without deviating much from the truth, which, perhaps, is all that may be neceſſary for the conſideration of the natural philoſopher. At all events, they may be of ſome uſe, for this end, in ſo many parts of the earth through which rivers paſs, and where no barometrical obſervations, or any others whatever, for taking heights above the ſea, have been, or perhaps ever will be made. They may alſo be found uſeful in taking the levels through a large extent of flat countries where regular canals and rivers paſs, and where the difference of elevation is too ſmall to be obſerved by the barometer, and where alſo the taking them through ſo great an extent by the common methods of levelling would be much too expenſive for the purpoſes required. Now in

this laſt caſe I have found, by experience, that by the method I here propoſe the difference of heights may very eaſily be found, and that very near to the truth.

For this end it may be proper to premiſe a few neceſſary conſiderations and precautions to be obſerved in making uſe of the method I here propoſe. They would eaſily occur to any one who conſiders the principles whereon it is grounded; but to ſave trouble I ſhall put them down in a few words.

81. The firſt is, that a particular attention muſt be had to the quantity of water actually in the river at the time of the operation, ſo that according as the greater or leſs quantity thereof may augment or diminiſh the velocity of the current, allowance may be made conformable thereto in determining the quantity of declivity from the degree of velocity.

2dly, Obſerving this precaution throughout the whole river, or all that part of it wherein we want to find the difference of elevations, we muſt next endeavour to determine, as near as poſſible, by the principles laid down in the laſt ſection, *all the variations of declivity from the variations of velocity within thoſe limits, and alſo the exact length and quantity of each.*

3dly, The ſame attention muſt be had in taking the difference of heights by canals, while their ſluices and

communications are kept conſtantly open, ſo as to effectuate a compleat natural current throughout the whole extent thereof; for in this caſe they are no other than rivers, and their waters follow the ſame laws of motion.

4thly, But in canals which are ſhut, and their waters kept up by ſluices ſo as to render them nearly ſtagnant, the practice of this method will be different from what it is in rivers and open canals: for in this caſe there cannot be allowed for the declivity of the ſurface of the water from ſluice to ſluice above one inch, or two at moſt, in each mile of length, according as there may be fewer or more accidental drainings of the water in it (N° 74.).

Again, as it may happen, in taking the levels of countries by the means of artificial canals, that the water in different parts may have different directions, attention muſt be had to *add* or *ſubtract* reſpectively the total declivity of each.

Moreover, it almoſt always happens, in canals where the ſluices are ſhut, that the water on the two ſides of each ſluice is of a very different height, the back waters being kept up, while the lower are run off to a certain point; but in ſluices next the ſea, the tide againſt the outer gates is ſometimes lower and ſometimes higher than the water in the canal above. In all theſe caſes, the difference of height muſt be exactly meaſured, and the

quantity

quantity reſpectively added or ſubtracted in the account of the levelling.

5thly, After this it is neceſſary to determine, as nearly as poſſible, the length of the canals and rivers through all their turns and windings, and throughout the whole extent of country in which we want the difference of elevations. This may be done by an actual menſuration, or by the general opinion of the inhabitants of each part of the country, which, being founded upon the long and continually repeated experience of an infinity of people, will be found to differ very little from the truth, *attention being had to the quantity of their nominal meaſures*; even the errors in *more* or *leſs* will nearly compenſate each other; or, finally, in great extents it may ſuffice to compute them from good geographical maps.

6thly, This being done by one or the other of theſe methods, it will be eaſy, from the quantity of declivity before determined for each part in particular, to find the whole quantity of declivity throughout the whole extent of country meaſured, or from any one part thereof to any other along the rivers or canals in queſtion, which are ſuppoſed to be continued without interruption from one place to the other. If to this be added the relative height of the country in each place compared with the level of the water in the part of the river or canal next to each,

we ſhall have very nearly the difference of elevation of thoſe two parts of the country. And thus the levels may be taken from the ſea through any extent of country, nay even through whole continents, as far as rivers or canals extend without interruption. Cataracts themſelves, ſuch as thoſe in the Nile and in the river of St. Lawrence, need not hinder the operation, ſince we have only to take the reſpective heights from which they fall into the account as we do in common ſluices, and allow for the increaſe of velocity produced by them in the current of the river above and below the places where they exiſt.

82. Although I do not pretend to equal this method (of finding the difference of heights in countries) *for exactneſs* to the levels taken by actual menſuration, or to thoſe found by a long ſeries of nice barometrical obſervations; yet it muſt be allowed, that it is free from many inconveniencies, and accompanied with many conveniences, which the others are not. It may be eaſily carried through great extents of country, where the other methods cannot be put in practice, on account of the expence or time required; and this may be done with very little trouble, and perhaps with ſufficient exactneſs to anſwer all the purpoſes of the natural philoſopher in his conſiderations on the globe we inhabit. Although the method of taking heights by barometrical obſervations

obſervations is highly uſeful, and among mountains (where mine can be of little or no ſervice) far preferable to every other hitherto diſcovered; yet it will eaſily be acknowledged by every one who is acquainted with what M. DE LUC [m], Sir GEORGE SHUCKBURGH, and Colonel ROY [n], have done upon this ſubject, that the greateſt attention to an infinity of varying circumſtances, as well as the greateſt nicety and exactneſs both in the inſtruments and in repeated obſervations, are neceſſary if we would come at the truth thereby.

Again, the method of taking the difference of heights by the quantity of declivity in rivers requires no attention to the curvature of the globe, an object (as every one knows) infinitely too conſiderable to be neglected in the common method of levelling; as are alſo the great and varying refractions of the viſual rays ſo near to the ſurface of the earth as they muſt be taken in the practice of that method. The quantity of effects and of errors in the viſuals proceeding from this laſt cauſe muſt be very different at different times, as it depends wholly on the greater or leſs denſity, on the greater or leſs quantity of vapours ſuſpended in the loweſt part of the atmo-

(*m*) See his work on Barometers and Thermometers, in two vol. quarto.

(*n*) See the learned and curious treatiſes of theſe two gentlemen in the Philoſophical Tranſactions for 1777.

ſphere,

ſphere, the ſtate of which ſeldom remains long the ſame. Now it is no eaſy matter either to determine the quantity of theſe exactly, or to calculate the effects and errors in the viſual rays proceeding therefrom, which yet muſt be done to come at the truth by the common method of levelling; whereas, in the method I propoſe, no ſuch conſiderations are neceſſary, as is evident from the nature of it.

But this is more than enough on a method ſo obvious and eaſy; I ſhall now give a few examples of it, and thereby conclude this eſſay, already perhaps much too long.

83. Suppoſing the length of the Scheld, between Antwerp and Ghent, following all its meanders, to be forty meaſured Engliſh miles, as it is reckoned nearly to be; and ſuppoſing the length of the ſaid Scheld between Ghent and Tournay to be fifty of the ſame miles; and that of the Lys from Ghent to Commines, where it approaches neareſt to the city of Ipres, forty-ſix miles; it is required to know the reſpective differences of elevation between all theſe places.

It may be found above (N° 78. 79.) that the river Scheld, between Ghent and Antwerp, has not above one foot declivity in each mile of its courſe; and that the

Scheld and the Lys, above Ghent, have about one foot declivity in each four thouſand feet of length.

According to this, the ſurface of the Scheld in Ghent is about forty feet higher that it is at Antwerp; and at Tournay it is ſixty-ſix feet higher than at Ghent, and one hundred and ſix feet higher than at Antwerp. So alſo the ſurface of the Lys at Commines is ſixty-one feet higher than at its junction with the Scheld in Ghent, and one hundred and one feet higher than the ſame at Antwerp. From hence it may be deduced, that the Scheld at Tournay is about five feet higher than the Lys at Commines, through twenty-five miles of interjacent country.

84. Suppoſe it be required to find the difference of height between the ſurface of the Lys at Commines and the ſurface of the canal at Ipres which falls into the ſea at Nieuport on the Coaſt of Flanders. The diſtance between Ipres and Commines is nearly ſeven meaſured miles, through which there is no communication by water; but there is one a great way round, which therefore, for the purpoſe required, muſt be followed through all the differences of elevation comprized therein, *viz.*

Deſcent

Deſcent towards the ſea at Nieuport *on the Coaſt of* Flanders.

	Feet.
Total declivity of the Lys from Commines to Ghent, 46 miles, - - - - - -	61
Declivity of the canal from Ghent to Bruges, when the ſluices are ſhut, - - - -	1
Difference of height of the water in the aforeſaid canal above that in the canal from Bruges to Oſtend, which two communicate together by ſluices, - - - - -	8
Declivity from Bruges to Plaſchendahl where the canal of Nieuport joins that of Oſtend, -	6
Total declivity from Plaſchendahl to Nieuport, including the difference of ſurfaces in two intermediate ſluices, - - - - -	8
Total declivity, all one way, from Commines to Nieuport, about 95 miles, - - -	84

Aſcent

Aſcent from the ſea at Nieuport *to the city of* Ipres.

	Feet.
Difference of height which the river from Nieuport to Ipres has above the canal from Nieuport to Plaſchendahl, taken croſs the harbour of Nieuport, by means of the tides which come up againſt the outer gates of the ſluices next the ſea on each,	3
Declivity of the river Iprelee from Nieuport to Boeſinghe, - - - -	11
Difference of level of the water above and below the ſluice of Boeſinghe, - -	$22\frac{2}{3}$
Declivity in the canal from Boeſinghe to Ipres, -	$\frac{1}{3}$
Total aſcent, all one way, from Nieuport to Ipres,	37

Now 84−37=47 feet for the difference of height which the ſurface of the Lys at Commines has above the ſurface of the canal at Ipres. I have made uſe of the above example preferably to any others, as it is very complicated, and becauſe the quantities of declivity which I have put down are not arbitrary; and, moreover, becauſe I had the good fortune to find, ſome years after I had taken thoſe meaſures, that I only differed two feet

from what was found by actual levels made from the Lys to Ipres by the French engineers during the time that LEWIS the XIVth was maſter of the country, when there were propoſals for opening a canal from the one to the other.

85. I ſhall venture to carry my conjectures ſtill farther, and grounding them upon the principles laid down above (N° 78. 79.) I ſhall take a general view of the elevations of continents along the courſe of the principal rivers in the known world. I cannot, however, repeat too often, that I give this as a matter of mere conjecture and curioſity. It has not been, nor ever will be, in my power, or in that of any other particular perſon whatſoever, to follow the courſes of all the rivers mentioned in the enſuing table from their mouths to their ſources. All that can poſſibly be done on this head, is to examine the relations of voyagers and geographers concerning each river as far as it is known, and to reduce it by that means within the compaſs of the hydrometrical principles laid down in this eſſay. This is what I have done as far as I could; and therefore, allowing that I make great miſtakes therein, yet I do not think that I merit much blame on that account, as I have done what I was able to do. If I am blame-worthy, it is for having launched out

too far into the regions of conjecture; but as ſuch miſtakes as theſe are no ways prejudicial to my fellow creatures, to whom I wiſh to be uſeful, and as they may give occaſion for others to rectify them, and ſo lead them to a ſubject which otherwiſe, perhaps, they might never have attended to, I ſhall hope for indulgence from all thoſe who wiſh well to humanity and to uſeful knowledge.

86. A table of the elevation of countries above the surface of the sea, at each 100 miles of length up the course of the principal rivers in the world, as far as they extend; by computation from the principles laid down in this treatise.

Feet of elevation in 100 miles.	Feet of elevation in 200 miles.	Feet of elevation in 300 miles.	Feet of elevation in 400 miles.	Feet of elevation in 500 miles.	Feet of elevation in 600 miles.	Feet of elevation in 700 miles.	Feet of elevation in 800 miles.	Feet of elevation in 900 miles.	Feet of elevation in 1000 miles.	Names of Rivers, And quantities whereby the length of their course is to be diminished, to have the distance from their mouths in a direct line.
100	210	330	470	630	820	1040	1300	1600	1950	The river Trent, the Meander, and many others of the same kind, which are seldom of great extent: in these one-third of the course may be allowed for its deviations from a right line.
150	310	480	670	880	1120	1400	1770	2160	2620	The river Thames; the Seine and the Somme in France; the Nile and the Niger in Africa; the river St. Laurence, the Oroonoko, the river of Amazons, the rivers of Paraguay: in these about one-fourth of the length of course may be allowed for turns and windings in it.
220	450	700	980	1290	1640	2040	2500	3030	3630	The Severn; the Loire and Garonne in France; the Tagus, Guadiana and Guadalquivir in Spain; the Po and the Tyber in Italy; the Meuse, Rhine and Elbe in Germany; the Weixel, Neister, Bog and Nieper in Poland; the Don and Dwina in Russia; the Amur in Tartary; the Hoang-ho-keou and Yang-tse Kiang-keou in China; the rivers of Cambodia, Ava and Ganges in India; the Euphrates; the Zaire in Congo; the Missisippi: in these may be allowed about one-fifth of the length of the course for turns and windings in it.
300	650	1050	1520	2070	2720	3500	4440	5570	6920	The Rhône in France; the Ebro and Douro in Spain; the Danube; the Wolga; the Irtisch, Oby, Jenesca and Lena in Siberia; the Malmistra in Cilicia; the Tigris; the Indus. The course of these rapid rivers is usually very straight, and there cannot be above one-fifth of the length thereof allowed for devistions from a right line.

XXXVIII. *Extract of two Meteorological Journals of the Weather, observed at* Nain, *in* 57° *North Latitude, and at* Okak, *in* 57° 30′, *North Latitude, both on the Coast of* Labradore. *Communicated by Mr.* De la Trobe *to the President, and by him to the Society.*

Read May 20, 1779.

THE thermometer, of FAHRENHEIT's ſcale, was obſerved at eight in the morning, at noon, at four in the afternoon, and at eight in the evening.

The barometer, whoſe ſcale is French meaſure, was obſerved at eight in the morning, and at eight in the evening.

Months.

Months.	Thermometer at Nain.			Barometer at Nain.			Thermometer at Okak.		
	Higheſt	Loweſt.	Mean.	Higheſt	Loweſt	Mean.	Higheſt	Loweſt.	Mean.
1777	°	°	°				°	°	°
Auguſt	76,0	36,0	52,3	28,3	27,6½	27,10,4	74,0	36,0	52,0
Septemb.	72,0	27,0	43,6	28,5½	27,6½	28,0,4	68,0	21,0	43,9
October	51,0	12,0	30,2	28,7½	26,11½	28,0,1	48,0	13,0	30,0
Novemb.	41,0	8,0	15,4	28,9	26,9	28,0,0	39,0	5,0	17,5
December	31,0	26,0	10,3	28,5½	26,3	28,3,0	32,0	28,0	4,2
1778									
January	18,0	30,0	12,2	28,5	27,1½	28,4,5	16,0	27,0	11,0
February	32,0	27,0	10,7	28,6	27,3	28,7,6	34,0	25,0	9,2
March	27,0	25,0	4,3	28,10½	27,2½	27,4,0	32,0	18,0	6,5
April	49,0	8,0	27,1	28,6½	27,6	28,4,3	53,0	10,0	28,5
May	68,0	29,0	39,0	28,5½	27,7	28,7,5	65,0	27,0	37,5
June	77,0	33,0	45,0	28,6	27,4	28,0,0	73,0	31,0	45,7
July	82,0	34,0	51,0	28,3½	27,8½	28,0,1	81,0	36,0	51,3
Auguſt	74,0	40,0	52,5	28,5	27,5½	28,0,0	72,0	41,0	52,0
Septemb.	69,0	34,0	43,5	28,0	27,10	28,2,0	53,0	32,0	43,0

IMPROVEMENTS

IN

ELECTRICITY.

By JOHN INGENHOUSZ, F.R.S.
BODY PHYSICIAN TO THEIR IMPERIAL MAJESTIES.

Who was nominated by the Preſident and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late HENRY BAKER, Eſq. F. R. S.

Read at the ROYAL SOCIETY, June 3, 1779.

IMPROVEMENTS

ELECTRICITY.

IMPROVEMENTS IN ELECTRICITY.

BEING appointed to deliver the annual diſſertation on ſome philoſophical ſubject, inſtituted by our worthy brother the late Mr. HENRY BAKER, I will endeavour to explain ſome contrivances in electricity which I invented a good while ago, one of which has been much employed, and has undergone ſeveral material improvements ſince I firſt thought of it.

IT is now about fifteen years ago ſince I began to make uſe of flat glaſſes inſtead of globes or cylinders, to excite electricity. Finding that a greater quantity of electricity could be excited upon a flat piece of glaſs, when rubbed on both ſurfaces, than when it was only expoſed to friction on one ſide; I thought it would be an advantage to ſubſtitute a round plate, or diſk, of glaſs, to a globe

or cylinder. I alſo thought another material advantage might be derived from a plate of glaſs, as the form of it admits of placing cuſhions or rubbers upon different parts of it, and taking the electricity, excited by theſe rubbers, from the interſtices between them, which cannot conveniently be done when a globe or cylinder is uſed. The only inconvenience which I at firſt conceived would enſue from it was, that the center upon which the plate was to be fixed and whirled round, would always be too near the rubbers (unleſs theſe were very ſhort, or the plate of a conſiderable ſize), and that theſe would throw the electricity, collected on the ſurface of the glaſs, upon that very center. In order to obviate this difficulty, I propoſed to make the center likewiſe of glaſs, or ſome other non-conducting ſubſtance, as, for inſtance, baked wood.

I began firſt by making uſe of one of thoſe glaſs ſtands, which they call a waiter, and which has a glaſs ſupport fixed at right angles to its center. I whirled the waiter round as well as I could, rubbing it ſometimes on one ſide, ſometimes on both. In this imperfect ſtate I ſhewed it to Dr. FRANKLIN, who approved much of the ſcheme, and adviſed me to purſue it. Soon after, I ſhewed it to ſeveral of my acquaintance, and in a ſhort time I found ſuch machines ready made at Mr. RAMSDEN's and ſome

other mathematical inſtrument-makers. Since that time great uſe has been made of theſe electrical plate machines throughout Europe, as they were thought by many to be more powerful in a little compaſs than thoſe with globes and cylinders.

In my travels through different countries, I now and then met with conſiderable improvements made in them. Abbé FONTANA had contrived one for the cabinet of the great Duke of Tuſcany, which conſiſted of two plates, eighteen inches diameter, fixed to the ſame center, and each rubbed on both ſides on two oppoſite places. The electrical fire excited on theſe joint plates, when forced upon a conductor divided in two branches to receive it, was very powerful; ſo much ſo, that the conductor, being unable to contain the whole, threw it back upon the braſs center; from which it paſſed to the hand of the operator, and gave him a very diſagreeable ſhock.

Mr. CUTHBERTSON, an ingenious mathematical inſtrument-maker at Amſterdam, contrived an apparatus with a double plate, by which all the electrical fire collected by the eight cuſhions was forced upon the conductor, ſo that none of it could be thrown back upon the center, though made of braſs. His contrivance conſiſted in placing between the glaſs plates a ſtrong glaſs ring, about two inches in diameter, ſo that the braſs

center paſſed through the middle of it. This ring was ſtuck to the plates with ſealing wax or ſome other non-conducting cement; and the ſpace between the center and the ring was carefully filled with the ſame non-conducting ſubſtance. The conductor had two branches, each of which was placed between the two glaſſes, reaching very near to the glaſs ring. By this method all, or almoſt all, the electricity excited by the eight cuſhions, was forced to paſs upon the conductor, there being no way to reach the braſs center, between which and the conductor all communication was cut off by the above mentioned glaſs ring being filled up with a non-conducting cement. The power of ſuch a machine, notwithſtanding that the plates were not above fifteen inches diameter, was very aſtoniſhing. I ſaw one made in London with this improvement (the glaſs plates being eighteen inches diameter) by which a coated jar of two quarts was fully charged in leſs than five ſeconds.

Mr. C. CUYPERS, an ingenious electrician at Delft, has not a little contributed to the improvement of theſe machines, by making them leſs liable to be affected by damp weather. This gentleman, conſidering that all glaſſes are not equally fit for electricity, and that J. H. WARTZ, and after him Profeſſor MUSSCHENBROEK, were of opinion, that glaſs, in the compoſition of which there enters

a great deal of alkaline ſalt, is very apt to attract moiſture from the air, and therefore leſs proper for electricity (which defect they thought might be corrected by expoſing it to a violent and continued heat), took a proper advantage of this knowledge in the improvement of the machines with flat glaſſes. He found, that glaſſes, which have been many years expoſed to the warm air of a room, very old looking glaſſes for inſtance, become by this means much harder, ſo as better to reſiſt the force of a file; and are then much better for electrical machines: and that ſuch glaſſes become ſtill incomparably better, if they are expoſed to a conſiderable degree of heat during ſome months: the heat forcing out of the glaſs (at leaſt out of its ſurface) the alkaline ſalt, not vitrified, which is to be found upon it, and may be known by the taſte.

In December 1777 I ſaw one of theſe double-plated machines at Mr. CUYPER's houſe, and found it do admirably well, though the weather was at the time very damp, and the machine kept in a room in which there never was any fire made, and though the cuſhions had no amalgama upon them: they were made of yellow Turkiſh leather, ſtuffed with fine ſhavings of cork, rammed in them; and had been preſſed to give them an equal ſmooth ſurface.

The

The ſame gentleman found glaſſes, prepared in the above mentioned way, far ſuperior in ſtrength to a cake of roſin uſed in the electrophorus. He publiſhed his method in a pamphlet, intitled, *Expoſé d'une Methode, par la quelle on rend les Diſques de Verre deſtinés à des Machines Electriques capables d'exciter l'Electricité dans une Air humide, ſuivi d'une Maniere de faire de très bons couſſins pour frotter les Verres des Machines electriques, et de la Deſcription d'un Electrophore perpetuel plus parfait que ceux dont on s'eſt ſervi juſq' ici.* A la Haye, 1778.

Thoſe who make uſe of plate machines ſhould carefully avoid putting the apparatus near the fire, for the purpoſe of drying or warming it; becauſe the ſudden expanſion of the glaſs by the heat cannot ſo quickly propagate itſelf through its whole extent; for the center being commonly ſqueezed between two flat ſhives of braſs, with a piece of leather between the metal and the glaſs, does not acquire a ſimilar degree of heat at the ſame time as the reſt, and cannot ſo eaſily expand; and therefore the plate is in great danger of breaking. If, in conſequence of ſuch a blunder, a flaw ſhould happen, its progreſs might be ſtopped by drilling a round hole at the extremity of the flaw. Theſe flat glaſſes may very ſafely be rubbed with a dry warm cloth.

As

As the quantity of electricity excited upon glaſs is nearly in the proportion of the ſurface expoſed to friction; and as glaſſes of a great ſize are very precious, and liable to accidents, I conceived, that inſtead of a disk of flat glaſs one might ſubſtitute one of paſte-board, thoroughly imbibed with copal or amber varniſh.

To try how this would anſwer, about ſeven years ago I ordered three paſte-board disks to be made, of four feet in diameter, the diſtance of ſix inches from the center being the fitteſt to give the whole a proper ſupport in whirling it round. When theſe disks were thoroughly dried and heated, I poured upon them a varniſh made of amber diſſolved in linſeed oil. After they had taken in as much of the varniſh as they could imbibe, I covered them with a thick coat of the ſame varniſh, and dried them by the heat of a German ſtove.

When the varniſh was very hard, I found, that even a ſlight friction with a cat's skin or hare's skin excited a ſtrong electricity upon them.

I then made a frame to place them in, and to whirl them round; which frame was ſo contrived, that it could contain about twelve ſuch disks, whirling all round on the ſame center. It conſiſted of two ſquare pillars of wood, about five feet high, and three inches broad; joined together at top and bottom by a tranſverſe piece

of

of wood. In the middle of the two pillars was a hole, about an inch and an half diameter, fitted to receive a wooden axis, which could be placed in, and taken out, at pleaſure. Upon this axis were to be ſtuck the paſte-board disks; and a flat board, three inches broad, covered on both ſides with flannel, and over this with a hare's skin, was to be placed between each paſte-board. The two ſquare pillars were alſo to be wrapped up firſt with flannel, and over that with hares skin.

The flat boards, to be placed between the paſte-board disks, had each a notch in the center, to give room for the axis to turn round freely. Theſe flat boards could be brought as near one another as was required by two wooden male ſcrews, placed at the upper and lower end of the frame, which reached from one ſquare pillar to the other; which ſcrews were to receive a notch cut out at the upper and under end of each flat board, in order to keep them ſteady in their vertical ſituation. A female ſcrew, turning upon theſe horizontal male ſcrews, was placed between each of the flat boards at their upper and under extremities, and ſerved to bring each of the disks as near in contact with the hares skins as was required to receive a proper friction.

The three paſte-boards were fixed in the frame, and whirled round. The electricity excited was ſo ſtrong that

that I took ſparks between one and two feet long from the front ſurface of the firſt diſk by approaching my knuckle to it. I then applied a tin conductor to it, about ſix feet long and ſix inches diameter, divided into two branches, the extremities of which were furniſhed with a thick ſilver lace fringe inſtead of points. The ſparks from this conductor were about four or five inches long, appeared to be very thick, were very brilliant, and ſo ſtrong, that I did not chuſe to receive many of them; nor did thoſe who came to ſee the machine care to receive more than one. As theſe ſparks ſucceeded one another at ſhort intervals, I think they would have been much longer if every thing had been adapted for that purpoſe; as I ſaw afterwards done at Mr. NAIRNE's, who contrived to obtain ſparks of twenty inches long and upwards from a large glaſs cylinder.

I conſidered this paſte-board machine rather as a rough ſketch of an apparatus, by which I conceived the hope of obtaining an electrical power of almoſt any degree required, than as a compleat machine. My intention was to find out a contrivance by which a very great quantity of electrical fire might be collected without great expence and without much danger of breaking the apparatus, which two articles cannot be avoided when you make uſe of uncommon ſize glaſs cylinders or diſks.

I ſaw, two years ago, at the Duke of CHAULNES at Paris, an apparatus which had a plate glaſs five feet diameter. This alone coſt him eight hundred French livres.

As I had not adapted the tin conductor to receive the electricity from the three disks, but only from the front disk, I cannot tell whether the force of electricity would have been proportionably ſtronger if I had made ſome metallic communication between each of the disks.

I found, that the apparatus, as it was conſtructed, could not eaſily have admitted more than three ſuch large disks; for the twelve ſurfaces expoſed to friction being each a foot and an half long, and above three inches broad, made ſo much reſiſtance to the working of the machine, that it required a ſtrong arm to work it.

Being ſatisfied with having found that by this, or a contrivance of the ſame kind, a much greater power could be excited than by the common glaſs apparatuſes; I did not chuſe to put myſelf to more trouble or expence to increaſe the ſtrength of its frame, or the number of disks.

I muſt obſerve, that ſuch a machine may be kept in good order in a heated room; but that it will ſoon loſe its force in countries where it is not the cuſtom to heat rooms as the Germans do. My paſte-board disks kept

very good during all the time I left them in a room conſtantly warmed, which was about two months; but, when they were placed in a cold room, they ſoon loſt their power, having probably attracted moiſture from the air. I cannot be ſure that the varniſh I got made for the purpoſe was of the beſt kind: I have reaſon to ſuſpect the contrary, and therefore I ſhould think that much better might be obtained in London.

Such paſte-boards might be poſſibly preſerved from attracting moiſture, by keeping them ſhut up in a box made on purpoſe, lined within with tinfoil. The moiſture might alſo be expelled again by placing them a good while in a heated room, or upon a baker's oven.

It ſeems, beſides, not improbable, that a kind of paſte-board might be made by ſticking together the lamina of paper (firſt thoroughly dried) with a good oil varniſh inſtead of common paſte, as this laſt never can be deprived of its watery particles without loſing its cohefive quality. A good kind of paſte-board might likewiſe be contrived by ſticking together ſilk cloth inſtead of paper.

I contrived a plated machine, the disk of which was made of baked wood and boiled in linſeed oil; but it did not anſwer near ſo well as the paſte-board disks.

I found the paſte-board disks not much leſs ſuſceptible of electricity before I had varniſhed them than they were afterwards; but they loſt their force again in a few minutes, and did not recover it till they were dried and heated again.

It is well known, that writing and brown packing paper, when warmed, may acquire a conſiderable electrical power by being rubbed with hares skin, a piece of wood, ivory, nay even (as I found by experience) with a metallic body.

As it ſeems to be a general law of nature, that all reſinous bodies, excited either with a poſitive or a negative electricity, are more tenacious of keeping it, or ſeem to part with it, as it were, with more reluctance than glaſs (as I have demonſtrated in a paper, read laſt year before the Royal Society); it is adviſeable, that the conductor of ſuch a paper machine be not furniſhed with metallic points, which being neceſſarily kept at a diſtance will not take away all the electricity; but that ſome flexible, conducting ſubſtance, as ſilver or braſs lace fringes, communicating with the conductor, be in cloſe contact with the excited ſurface.

As woollen cloth or Manchester cotton velvet, and ſuch like ſubſtances, excite a good deal of electricity upon dried paper and reſinous ſubſtances, and do not wear out

ſo

ſo ſoon as hares skin, it might, perhaps, be found better to ſubſtitute them for hares skin.

I have alſo excited a very conſiderable electrical force on ſtrong ſilk velvet, tied upon the circumference of two wooden disks, two feet in diameter, and diſtant about three feet from one another, fixed upon a wooden axis. The velvet was ſupported by a ſtrong ſilk cloth tied under it, in order to give it more ſtrength and ſteadineſs. This machine had the appearance of a drum, and was whirled round, as is uſually done with glaſs cylinders. The rubber was a cuſhion covered with hares skin.

As ſilk cloth, and more particularly oiled ſilk, very eaſily receives a ſtrong electricity, I make no doubt but a good uſe might be made of them, by expoſing a great ſurface of them (which may be as large as one pleaſes) to friction. I have attempted more than one method of conſtructing ſuch a machine; but as I tried it only in ſmall, I have not purſued the object far enough, and therefore, I think, I have no right to throw out hints unſupported by experience.

[illegible]

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AN INDEX TO THE SIXTY-NINTH VOLUME OF THE PHILOSOPHICAL TRANSACTIONS.

A.

ACID. More of the vitriolic in gypſum, than in ſelenitical ſpars, p. 17.

Æther, how to produce inflammable air with the vitriolic, p. 384. Air from it does not evaporate as the æther itſelf, p. 388. Its explosive force encreaſed by camphire, p. 389. Great explosive force of very pure dephlogiſticated air, and æther, p. 407. Reaſon why a drop of it communicates to dephlogiſticated air a much ſtronger explosive force than common inflammable air from metals, p. 414.

Air. Difference between Dr. Prieſtley and Mr. Sheele about the noxious effects of the inflammable, p. 336. Animals killed by breathing it, when the receivers that contained it were immerſed in quick-ſilver, p. 338. Has the ſame effects when paſſed through water, as when ſhaken in water for a minute's time, p. 339. But may be breathed when ſhaken in it for a long time, p. 340. Kills quadrupeds not ſo readily as birds, ibid. Mr. Sheele's breathing it may poſſibly be accounted for, by its being breathed through the lungs, and not the noſe, ibid. Experiment proves the contrary, p. 341. Experiment to prove that inflammable air does not act upon the body of the animal, p. 342. Explanation of Abbé Fontana's formula to expreſs the diminution of reſpirable air, p. 343. Experiment to ſhew inflammable air worſe than common, p. 344, &c. Inflammable, better after being breathed than before, p. 348. Air from the lungs alters the nature of inflammable air, p. 349. Animals affected by inflammable air, more or leſs, according to their ſize, compared with the quantity of air, p. 351. Inflammable air ſtill inflammable after being breathed, contrary

trary to Mr. Sheele's aſſertion, p. 351. The light ſenſation on breathing inflammable air owing to its ſpecific levity, p. 352. Air lightened by fire breathed more eaſily than more condenſed air, p. 353. Why inflammable air can be breathed when mixed with common air, p. 355. Inflammable lighter than common air, and does not mix with it, p. 357. Account of the ſparks which dart from the inflammable air of metals applied to a candle, p. 359. And which are the diſtinctive quality of inflammable air extracted from metals, ibid. Phlogiſtic principle more combined with this air than with the others, p. 360. Analyſis of the decompoſition of the inflammable air of metals, p. 361. Great expectations from the uſe of the dephlogiſticated air as a remedy, p. 377. Large quantities of it extracted from water which has ſtood ſome time expoſed to the ſun, ibid. Reaſon why gunpowder does not want a free acceſs of common air to be conſumed by fire, p. 404. Uſes which may be derived from the ability to produce any quantity of dephlogiſticated at pleaſure, p. 416. The uſual ſtate of it in the Hartz mines a mean among the different ſtates of the exterior air as found by Mr. de Luc's obſervations, p. 495. Many modifications of it beſides its weight and heat to be conſidered, p. 496. Difference of the effects of heat on it one great cauſe of the diverſity of reſults in experiments, p. 498. Neceſſity of multiplying meteorological inſtruments to determine its influences, p. 501. Experiments on air extracted from the water of a well, p. 432. That extracted from the water of the river Seine, ſhewn to be better than atmoſpheric air, p. 434. Extracted from the water d'Arqueil, experiments on it, p. 436. Is ſhewn to be better than the air extracted from Seine water, p. 436, 437. That of diſtilled water extracted, and ſhewn to be better than that of the water d'Arqueil, p. 437. New characteriſtic by which dephlogiſticated air may be diſtinguiſhed from common, p. 439. A greater quantity of thoſe airs which contain the leaſt phlogiſton abſorbed by water, ibid. Impoſſible to determine the quality of it when extracted from veſſels filled with water by means of fire, ibid. Reſult of experiments on water deprived of air, p. 440. Water diminiſhes the noxious part of tainted, and dephlogiſticates common, ibid. How to prevent it being altered by the vapour of the water, in the above experiments, p. 442. Common air ſhaken in water is increaſed in bulk, ibid. Dephlogiſticated is decreaſed, p. 444. Which ſhews that this laſt differs eſſentially from common air, ibid. Great nicety required in theſe experiments, p. 445. Experiments made to aſcertain the ſalubrity of the atmoſpherical in various places, countries, and ſituations, not to be depended upon, p. 446. Difference of the air between one country, or any particular ſituation, and another, not ſo great as is commonly imagined, p. 447. Air taken from different parts of Paris and London examined, and found not to differ materially, p. 447, 448. Differences between the changes of air at the ſame place at different times, much greater than the difference of air of different

places or heights, p. 449. Uſes to be derived from this method of examining air, p. 452. See *Gaſs*.

Air-Gun. Account of a braſs one contrived by Mr. Maty of Turin, p. 392. And of one by Mr. Antony, p. 393.

Algebra. Problems concerning interpolations, p. 59. On the general reſolution of algebraic equations, p. 86.

Allum. Tobacco-pipe clay fitted for the making of it, p. 19. Quantity of, afforded by the Porcelane clay from Cornwall, p. 21. How made of feld-ſpar, p. 23. And of the Labradore ſtone, ibid. And of ſhells, p. 24. Obtained from different ſlates, p. 25.

America. The growth of plants in, not to be eſtimated by what we ſee in our hot-houſes, p. 250.

Annulus. Obſervation of a luminous, ſurrounding the diſk of the moon, in an eclipſe of the ſun, ſuppoſed to be the effect of the lunar atmoſphere, p. 106—111.

Anticonductor. Account of an anticonductor, and its advantages in exhibiting electrical experiments, p. 454. See *Electricity*.

Antimony. A liver of, from antimony and ſeveral ſpars, p. 19.

Antiquity. Account of a gun which had remained 200 years in the ſea, p. 40.

Apes. Probable that there is a ſpecies of them in Africa, which is not the ancient Pithecos, p. 147. See *Orang-Outang*.

Aphonia. A concomitant of the St. Vitus's dance, p. 2.

Argentum vitreum. See *Silver*.

Aſs. Foal aſs killed to examine the parts of generation, p. 282.

Aſtronomy. Obſervations at Cork in Ireland, p. 163. Obſervations with an aſtronomical clock, p. 167. Latitude of Cork in Ireland, p. 165. 167. 180, 181. Longitude of Cork, p. 167. Latitude and longitude of Cork, ſettled from obſervations at Cork, and at the Royal Obſervatory, p. 179.

Atmoſphere. Colour occaſioned by the refraction of, when a ſtar is but a few degrees above the horizon, rather more than corrected by the refraction of the lens, added to the equatorial inſtrument, p. 335.

B.

Barometer. Obſervations made on it at the coaſt of Labradore, p. 652. Some barometrical experiments in the mines of the Hartz, p. 484. Their agreement with the geometrical meaſure, p. 491. Why the barometrical meaſure for the depth of mines is more certain than that for the heighth of mountains, p. 492. State of, at London for each month throughout the year 1778, p. 296. Greateſt, leaſt, and mean

mean heighth of it there, in each month, p. 320. State of the barometer at Lyndon, in 1778, p. 547.

Beaumé, Mr. miſtaken in ſome chymical aſſertions, p. 11—20.

Belidor. Miſtakes in his theory of Vaulone's pile engine, p. 121. And in the ſolution of a problem on the ſubject, p. 126.

Bliſters failed to relieve in a St. Vitus's dance, p. 3.

Bones. Head of the os humeri ſawed off, and the motion of the limb preſerved, p. 6. Treatment of a diſeaſe of the lower parts of the ulna, where it joins the carpus, p. 10. More than three inches of the enlarged bone ſawed off, where the ulna joins the carpus, ibid. Diſeaſes of other joints recommended for cure by the ſame operation, ibid.

Bread-fruit. See *Sitodium*.

Breaſt. Eruption on the, relieved by ſalt-water, p. 54.

C.

Canal. See *River*.

Cane. See *Sugar cane*.

Catoptric. Advantages of a new catoptric micrometer, p. 421. See *Micrometer*.

Cauk. Experiments on the Derbyſhire, p. 17. Melted with antimony has appearances of Rulandus's falſe liver of antimony, p. 19.

Cementing cryſtalline particles the more immediate cauſe of the conſolidation of all ſtones and marbles whatſoever, p. 8.

Charcoal. Reflections on the different methods of reſtoring to life thoſe choaked by, p. 389.

Chirurgical improvements by Mr. White, p. 6.

Clay. Change of it into feld-ſpar, and different gradations of the change, p. 12. Diſtilled with ſea-ſalt produces a ſal ammoniac, p. 21. Quantity of allum afforded by the porcelane clay from Cornwall, ibid.

Cæcum. Vermicular proceſs of the inteſtinum cæcum in the Angoleſe and Aſiatic Orang, and likewiſe in the Gibbon, unknown to Galen, p. 148.

Comet. Periodical time of that of 1770 inveſtigated, and found not to be above five years and a half, p. 68. Argument on which this concluſion is founded, thought by the author equivalent to geometrical demonſtration, p. 69. It conſiſts in the aſſumption of elements anſwering to a periodical time of five years and ſeven months, which are found perfectly conſiſtent with the obſervations of this comet, ibid. Its not appearing according to this hypotheſis probably owing to the influence of Jupiter upon its orbit, p. 82. Table of what part of the heavens it is to be looked

looked for when it appears next, p. 83. Note on this subject by the Astronomer Royal, p. 84.

Conductors. Account of an anticonductor, p. 454. Its advantages for exhibiting experiments analogous to the natural effects of lightning from the clouds, p. 453. Experiments to shew the advantages of pointed over blunt conductors, p. 46.

Cork in Ireland, latitude of, p. 165. 167. 180, 181. Its longitude, p. 167. Its longitude farther settled from observations at Cork, and at the Royal Observatory, p. 179.

Cow-calf. Account of a species of, called the Free martin, p. 285.

Crotch, William, the musical child, account of him, p. 187.

Crystal. Partly changed into selenitical spar, p. 12. The change owing to the lead ore which adheres to it, p. 14. How known from other substances with a sparry appearance, p. 27.

Curves. Demonstration of them suited to the purposes of optics, recommended to mathematicians, p. 425.

Cuthbertson, Mr. Electrical apparatus, with a double plate, contrived by, p. 663. Its amazing powers, p. 665.

Cuyper, Mr. His improvements in the double-plate electrical machine, p. 665.

Cynocephalus. See *Orang Outang.*

D.

Dipping. See *Needle.*

Dropsy. Extraordinary one, from a disease in the left ovary, p. 57. Woman tapped 155 times, and 465 gallons of water taken from her, p. 58.

E.

Earth. Problem to determine how much each particle of it is affected by the unequal action of the sun, p. 510.

Eclipse. Total, with duration, and annular of the sun, observed on the 24th of June, 1778, p. 105. Observation of the luminous annulus surrounding the disk of the moon, p. 106. This last appearance owing to the lunar atmosphere, p. 111. Point of the sun's disk seen before its limb began to emerge, p. 113. Solar spots seen during this eclipse, p. 117. Eclipse of the sun observed at Cork, p. 178.

Eddies and whirlpools, what they proceed from, p. 607.

Edwards, Mr. Mistake of his in the representation of the Orang Outang, p. 146.

Electricity. Cure of the St. Vitus's dance by, p. 1. Pulse quickned, and an eruption like the itch produced in all the joints, by positive, p. 4. An antispasmodic, p. 5.

Electricity. Improvement of the apparatus, p. 454. Account of an anticonductor, p. 454. Advantages of this apparatus, for exhibiting experiments more analogous to the natural effects of lightning from the clouds, than it is possible to do with only one conductor positively electrified, p. 453. Experiments to shew that the two conductors are differently charged, as soon as the cylinder is put in motion by turning the wheel, p. 456. Experiment to shew the possibility of compressing the electric matter, tho' the globes are perfectly insulated, p. 488. Experiments to shew the advantages of pointed over blunt conductors, p. 460.

Electricity. Account of an electrical plate machine, p. 663. Flat glasses used to excite it, instead of globes or cylinders, p. 661. Paste-board imbibed with copal or amber varnish substituted to the glass disks, p. 667. The machine described, and instances of strong electricity excited with it, p. 669. Machines of this kind lose their power by being kept in cold rooms, 671. How to preserve them, or restore their powers when lost, ibid. The conductor of a paper machine must not be furnished with metallic points, p. 672. Account of a plated machine, with a disk of baked wood, boiled in linseed oil, p. 671.

Equatorial. Apparatus applied to the equatorial instrument for correcting the errors arising from the refraction in altitude, p. 332. Inconvenience of the method hitherto used, p. 333. Uses of a spirit level fixed to the equatorial instrument, p. 334.

Equinoxes. On the precession of them produced by the sun's attraction, p. 505. Precession different, according as the ring at the equator is considered as fluid, or as hard and compact, p. 506—509. Problem of the precession of the, requires no principles but the received doctrine of motion, and the application of the lever, p. 509. Problem to determine how much any particle of the earth is affected by the unequal action of the sun, p. 510.

Eric Prosperin, Mr. shewed that the observations of four months on the comet of 1770 could not be represented by a parabolic orbit, p. 84. Conjectured from thence that its orbit might be sensibly elliptical, p. 84.

F.

Fold-spar. Change of clay into, p. 22. The Labradore stone one, p. 25. How distinguished from crystal and quartz, p. 27.

Fontana, Abbé. Electrical plate machine contrived by, p. 663.

Forfait, Mr. a memoir of his, to indicate the best and cheapest methods of freeing navigable canals from banks of sand and earth formed in their beds, which render them too shallow; recommended, and an account given of it, p. 598.

Free

Fox Martin. Account of the, p. 279—285. Purposes for which they are preserved, p. 285. Their resemblance to oxen, p. 285, 286. The supposed excellence of their meat not a property of them universally, p. 286. Dubious whether there are any among sheep, p. 287. Account of Mr. Arbuthnot's, p. 289. Mr. Arbuthnot's more deserving of the name of hermaphrodite than the two following, p. 290. Account of Mr. Wright's, ibid. Of Mr. Wells's, p. 292.

Frog. Analogy of the organ of voice of the Orang Outang with that of frogs, p. 156.

Frost. Russian method of restoring circulation in a frozen limb, by means of friction with snow, p. 330.

G.

Galen. Exactness of almost all his descriptions of the Cynocephalus, p. 141. Probable that he dissected an Asiatic Orang, p. 148. Vermicular process in the intestinum cœcum of several species of Orangs, unknown to him, ibid. Comparison of the organs of voice of the Prince of Orange's celebrated Orang, with Galen's descriptions, p. 154.

Gas. Easy method of producing a sufficient quantity of, without trouble or any particular apparatus, p. 380. Produced by mixing equal quantities of oil of vitriol and spirit of wine, and applying heat to the phial containing the compound, ibid. Advantages of this last gas above others, p. 381. Specific quantities of the different gasses examined, p. 382. One made of vitriolic æther, p. 384. Its properties, p. 386. And differences from the other inflammable air, ibid. Experiment to prove it specifically heavier than common air, p. 387. Cautions about firing the pistol to make it, p. 390. Why it cannot exert the force of a wind gun, p. 399. Experiment to shew that its compound with common air is much reduced in bulk after inflammation, p. 400. Great explosion of very dephlogisticated air and æther air, p. 407. See more under *Air*.

Generation. What causes the perfection of, p. 279.

Geometry. Horizontal Isochronic curve described, p. 583.

Glass. In which there is much alkaline salt, less fit for electricity than others, p. 665. Long exposed to heat, superior in strength for electrical purposes to a cake of rosin, used in the electrophorus, p. 666. Disks made with it must not be dried by the fire, ibid.

Gold and silver, found in their metallic form, mixed in a large proportion in an ore from Norway, p. 531.

Gold Pyrites examined, p. 528. And found seldom to contain gold, p. 528. Experiments on a piece of gold ore, which turned out to be gold in its native form, and not mineralized, p. 531.

Grenades. Obſervations on the climate of the, p. 216. Rain fallen there from June 1772, to June 1773, p. 217.

Gunpowder. Its force generally attributed to the ſudden extrication of a great quantity of permanent aërial fluid, within a narrow ſpace incapable of containing it, p. 391. This elaſtic fluid ſuppoſed by Robins to contain 250 times the bulk of the powder, ibid. Opinion of Count Saluce on this ſubject, p. 392. Uſes to which the extrication of ſo much permanent aërial fluid has been put by philoſophers, p. 392. Account of a braſs air gun, contrived by Mr. Maty of Turin, ibid. And of one by Mr. Antoni, p. 393. What the difference between the quantity of elaſtic fluid obtained by Robins, and others, may have been owing to, p. 393. Bernoulli's calculation of it probably neareſt the truth, but proves the juſtneſs of that of the others, p. 394, 395. Nature of gunpowder explained in a different way from what it has hitherto been, p. 395—397. Nitre and charcoal ſufficient of themſelves to make very good, p. 397. Its force owing to the extrication of the dephlogiſticated air from the nitre, and of the inflammable air from the charcoal, by the fire which inflames it, p. 398. Difference of the inflammation of gunpowder, and of dephlogiſticated and inflammable air in the air-piſtol, what owing to, ibid. Probable that the dephlogiſticated and inflammable air, extricated in the firing of it, undergoes a diminution by its inflammation, p. 401. Agreement of this ſuppoſition with the analyſis of gunpowder, as made by Abbé Fontana, p. 402. Accurate calculation of the expanſion of, a very difficult undertaking, p. 403. Reaſon why it does not want a free acceſs of common air to be conſumed by fire, p. 404. Why a ſingle ſpark of fire propagates the combuſtion throughout it with great rapidity, p. 405. Reaſons for thinking modern philoſophers would have diſcovered it, p. 406.

Gypſum. More vitriolic acid in gypſum than in ſelenitical ſpars, p. 17. How diſtinguiſhed from other ſubſtances of the ſame appearance, p. 28.

H.

Hartz. Barometrical obſervations in the mines of the, 486—502.

Hermaphrodites divided into two kinds, the natural and uncommon, p. 280. Account of the natural, p. 281. Of the unnatural, ibid. One part of each ſex which they do not poſſeſs, p. 281. Such hermaphrodites appear at firſt view to be females, and are very frequent among horſes, p. 282. Deſcription of one of this ſort, p. 281. In general would apppear an object of chance, p. 284. Circumſtance in the production of hermaphrodites in cattle, which ſeems to be an eſtabliſhed principle in the œconomy of propagation of that ſpecies of animal, and not a production of chance, p. 284.

Horſe. Account of the diſſection of an hermaphrodite, p. 282.

Hunter, Dr. Aſſay of ſeveral ores in his muſeum, p. 527.
Hygrometer. Obſervations with Mr. Smeaton's, p. 167—175.

I.

Jaſpers, how formed, p. 25.
Iconantidiptic. See *Teleſcope*.
Incruſtation, a particular one deſcribed, p. 41. How formed on the windows of Ruſſian houſes, p. 326. Dangerous effects of ſuch, ibid.
Inundations. See *Rivers*.
Iron, and its ſolutions, contribute to haſten and promote the progreſs of the concretion and induration of ſtone, whenever they meet with cementing cryſtalline particles, which abound in ſea-water, p. 38. Any induration, or petrefaction of matter, much haſtened, and the conſolidation rendered much more compleat by being near a maſs of iron, ſtill more by the admixture of any ſolution of that metal, p. 39. Confirmation of this doctrine by three inſtances, p. 40. 41. 43. 45. Dr. Fothergill's conjectures on this ſubject, p. 47. Common ſea ſand, with a ſmall mixture of the ſolution of iron, may become a very uſeful ſtone, p. 48.
Iron ſpathoſe ores, account of, p. 29.
Jupiter. Eclipſes of the ſatellites of, obſerved at Cork, p. 165—177. At Greenwich, p. 179.

L.

Labrador ſtone, account of the, and of allum made with it, p. 23.
Lead. Account of the white lead ore, p. 29. Tobacco-pipe duly prepared, propoſed to be uſed by painters inſtead of white lead, p. 20.
Lenſes, a concave, and a convex, applied before the object-glaſs of the teleſcope of the equatorial inſtrument, to correct the errors ariſing from the refraction in altitude, p. 333.
Level. Spirit level fixed to the equatorial inſtrument, its uſes, p. 334.
Levels. General and eaſy methods of taking them through large extents of countries where rivers paſs, p. 643.
Lift. Difference in the modes of reſtoring to it thoſe who are deprived of it by water, and thoſe who have loſt it by the fumes of charcoal, p. 329.
Lightning. Its effects on board the Atlas, p. 160. Struck the main-maſt head, deſcended down the rigging, and entered the gun-deck, ibid. Man ſtruck dead by, p. 161. No damage done to the ſhip, maſts, or rigging, nor any viſible track of the lightning to be found on the maſts, ibid.
Load-ſtones, artificial, how made by Dr. Knight, p. 51.

M.

Macrocarpon. See *Sitodium.*

Magnetic variation obſerved at Cork, p. 178. See *Needle.*

Magnetiſm. A thin cylindrical tin tube ſuſceptible of a conſiderable degree of, p. 543.

Marbles continually forming in the earth, p. 47. Their conſolidation, how effected, ibid.

Meteorology. Neceſſity of multiplying inſtruments to determine the various influences of the air, p. 501. Meteorological journal for 1778, kept at the houſe of the Royal Society, p. 596. Meteorological obſervations on the coaſt of Labradore, p. 657. Journal for 1778, kept at Briſtol, p. 551.

Mica. How known, p. 29.

Micrometer. Imperfections of that which depends on moveable parallel wires, p. 419. The uncertainties in the obſervations made with a divided object glaſs one, inveſtigated, and found to depend on its principle, p. 420. Advantages of a new catoptric one, 421. Its conſtruction and properties, p. 422. Method of adjuſting and uſing it, p. 425. Deſcription of a new one, ſuited to the principle of refraction, p. 428. Its ſuperiority to the object-glaſs micrometer, what it conſiſts in, p. 429.

Mines. Account of ſome barometrical experiments in the mines of the Hartz, p. 488. Barometrical obſervations in ſeveral of theſe mines, p. 502.

Mouet, Mr. Cauſe of his error in ſuppoſing that the ſemitranſparent ſpar contains ſulphur, p. 15.

Montmartre. See *Plaſter.*

Moon. Obſervations of a luminous annulus ſurrounding her diſk in an eclipſe of the ſun, p. 106. Effect of her atmoſphere, p. 111.

Muſic. Account of an infant muſician, p. 183. What a ſupernatural diſpoſition to muſic in infancy conſiſts in, p. 185. Hiſtory of William Crotch, the infant muſician, p. 187. Aſtoniſhing properties of his ear, p. 197. His excellencies, what they conſiſt in, p. 196. His powers of tranſpoſing, at two years and four months old, whatever he played, p. 199. Account of muſical prodigies more advanced in age, p. 201, 202. 204. Conjectures on what William Crotch may mature into, p. 204. Experiments propoſed to be tried with him, ibid. Abſurdity of leaving him to himſelf, without further teaching, p. 205. Evident that he would like the plaineſt muſic beſt, if different genera and diviſions of the muſical ſcale were tried upon him, ibid. Not all children ſuſceptible of being taught muſic, at leaſt not in the cradle, p. 206. Primitive powers in muſic ſometimes become ſtationary, the reaſons for it aſſigned, ibid.

Mozart. His talents for muſic at eight years old, p. 204.

N.

Needle. Journal of the variation-needle, kept at Royal Society's houſe, for a fortnight, p. 321. Journal of the dipping needle for a fortnight, p. 323.

Needles. Account of ſome new methods of ſuſpending magnetical, p. 537. Inconvenience of the beſt executed ones, with reſpect to the facility with which they are dragged out of their direction, p. 537. Remedied in different ways, p. 539. By putting them under water, p. 539. Thin ſteel tubes recommended for this purpoſe, p. 544. Inconveniences of common water for this purpoſe, and recommendation of expreſſed oils, p. 545.

Nitre. Why it explodes when combuſtible ſubſtances are thrown upon it, p. 396.

O.

Orang-Outang. Account of the organs of ſpeech of the, p. 139. Exactneſs of almoſt all Galen's deſcriptions of the Cynocephalus, p. 141. Why this animal cannot modulate his voice ſo as to articulate, p. 142. The meatus, or proceſſus peritonæi, cloſed in the Simia caudata imberbis, cauda ſubprehenſili, corpore fuſco, pedibus nigris, p. 143. Two papiones or ſphinges of Linnæus diſſected, ibid. Seven examined by Profeſſor Camper, p. 144. Want of nails on the great toes of the feet, and of the ſecond phalanx of the great toes, a remarkable character in theſe animals, p. 146. A little nail and two phalanxes ſeen on the great toe of one, ibid. Singular red long hair, and ſhortneſs of the neck, another very peculiar property, ibid. Mr. Edwards miſtaken in the repreſentation of the Orang-Outang, ibid. The figures of the Orangs, deſcribed by Tulpius and Tyſon, deficient in many reſpects, p. 147. Probable that Aſrica furniſhes a ſpecies of apes which are not the Pitheci of the ancients, ibid. Probable that Galen diſſected an Aſiatic Orang, p. 148. Vermicular proceſs of the inteſtinum cœcum in the Angoleſe and Aſiatic Orang, and likewiſe in the Gibbon, unknown to Galen, ibid. The organs of ſpeech deſcribed, ibid. The ventricles of one united, ſo as to form but one, p. 150. Quere, whether they grow together thus, or whether this be a variety? ibid. Hiſtory of the Orang belonging to the Prince of Orange, which died in 1777, p. 151. Account of the internal part of the organ of voice, and compariſon of it with Galen's deſcription, p. 154. Reaſon why this animal cannot ſpeak, p. 155. Analogy of its organs of voice with that of frogs, p. 156.

Ores. Aſſay of ſeveral in Dr. Hunter's muſeum, p. 527. See *Silver.*

Os humeri. Head of one ſawed off, and yet the motion of limb preſerved, p. 6.

Ovary. A diſeaſe in the left, the cauſe of a dropſy, p. 57.

P.

Pulſchan. Account of a German boy of that name, with great talents for muſic, p. 201.
Petrefaction. Account of one, found on the coaſt of Eaſt Lothian, p. 35. A piece of rope that was adjoining to an iron ring, with ſand ſo concreted round it as to retain impreſſions of parts of the ring, p. 36. Incruſtation formed within three years in a ſquare wooden pipe, in a coal mine in Somerſetſhire, p. 41. A progreſſive induration of maſſes of ſand, and other matter, at the bottom of the ocean, on the coaſt of this iſland, as at the bottom of the Adriatic, p. 37.
Piles. Theory of a new machine for driving them, p. 120. Advantages of that invented by Vauloüé, p. 121. Miſtakes in Belidor's theory of Vauloüé's machine, ibid. A new theory of it propoſed, p. 122. Uſeful theorems for comparing different machines, and determining the coſt of the works to be raiſed, p. 124. Problem to determine the depth to which a pile will ſink at each ſtroke of a given machine, ibid. Belidor's miſtakes in the ſolution of this laſt problem, p. 126. Miſtakes of a writer in the Stockholm Tranſactions concerning it, p. 126. Several corollaries uſeful in practice, p. 127.
Piſtol One for making inflammable air, p. 381. Very ſtrong one, for firing dephlogiſticated and æther air, p. 410.
Plants. Their growth in America not to be eſtimated by what we ſee in our hotheuſes, p. 250.
Plaſter. Compariſon of the Paris Montmartre plaſter ſtone, with ſeveral ſelenitical ſubſtances, p. 16.
Pulvis fulminans, a new theory of, p. 404.
Pyrites. Aſſay of gold Pyrites, p. 528. Seldom contains gold, ibid. Has been thought to contain gold united firſt with iron, and that compound united with ſulphur, ibid. Experiments on a piece which turned out to be in its native form, and not mineralized, p. 531.

Q.

Quartz. How known from other ſubſtances with a ſparry appearance, p. 27.

R.

Rain. Account of that fallen at the Grenades, from 1772 to 1773, p. 217. Regiſter of, at Lyndon, for 1778, p. 547.
Rattoon. See *Sugar-Cane.*

Rivers. Eſſay on rivers and canals, p. 556. Importance of the ſubject, ibid. Account of authors who have demonſtrated the principles laid down by Mr. Mann, p. 559. Theory of rivers and canals, p. 563. Definitions, ibid. Laws of action in rivers and canals, p. 564. How to find whether the water, in a part of a river where the bed is nearly horizontal, flows by the velocity acquired in the preceding declivities, or by the compreſſion of the upper water on the lower in that place, p. 568. How to determine the velocity of each particle of water in a regular channel, p. 569. The nature of rivers and flowing waters conſidered, p. 571. Cauſes of the acceleration of motion in rivers, ibid. Cauſes of its retardation, p. 572. Principles for calculating the quantity of the percuſſion of the waters of a river againſt an obſtacle oppoſed to their motion, p. 574. Greateſt and leaſt velocities of rivers, in what parts to be found, p. 576. How to meaſure the velocity of the current of a river, or open canal, p. 577. Application of the laws of the acceleration and retardation of currents to rivers and canals in general, p. 579. How to augment or diminiſh the velocity of water in rivers, p. 580. The perpendicular compreſſion of the upper waters upon the lower augments, as all the other cauſes of motion in rivers diminiſh, p. 581. The junction of many ſtreams in the ſame bed to be attributed to the wiſdom of the Supreme Being, p. 582. How to dig the bed of a canal ſo that the velocity of the current ſhall be every where equal, p. 583. The depth of a river can only be augmented to a certain degree, in proportion to its breadth, without hurting the banks and weirs made to keep it in, p. 584. Cauſes which contribute to fill up the beds of rivers, and produce inundations, p. 586. Sluices themſelves will produce theſe accidents, if the floors of their bottoms are not of a depth conformable to the bed of the canal, p. 588—591. How the principal accidents to which rivers and canals are liable may be prevented, p. 590. Great care, in digging the beds of rivers, muſt be had as to the quantity and form of their declivity, p. 594. Which muſt be increaſed in equal ſpaces, the farther we recede from their mouths, p. 595. One foot perpendicular of declivity to be allowed through ten thouſand feet of horizontal extent, ibid. Six machines propoſed by Mr. Forfait of Rouen, for removing banks formed in navigable canals, p. 598. Other conſiderations on the nature of rivers and inundations, p. 599. Rivers and canals have their beds raiſed, and their currents ſtopped by the ſubſiding of mud and heterogeneous matter in different places, eſpecially juſt above their ſluices, p. 600. Line of greateſt current in a river or canal defined, and the changes in the beds of rivers, ariſing from its directions from the centers of the ſections through which it paſſes, enumerated, p. 601. Why inundations are more frequent, conſiderable, and do more damage in the interior parts of a country than towards the mouths of moſt rivers, p. 604. Variations in the mouths by which they diſcharge their waters into the ſea enumerated, together with the changes produced by them, p. 605. Effects of counter-currents, iſlands

iſlands in the middle of rivers, eddies and whirlpools, p. 607. Inconveniences of diminiſhing the velocity of the current, by widening the canal, and method of obviating thoſe inconveniences, p. 608. Nature of inundations, and manner of their formation conſidered, p. 609. On the confluence of rivers, and on the ſeparation of the ſame river into divers branches or mouths, with the effects thereof on the velocity of currents, inundations, &c. p. 611. Great rivers receive many others into their beds, p. 612. This confluence uſeful for many purpoſes, ibid. One river may fall into another of equal magnitude with itſelf, and yet the ſection of the current in the common bed, after their confluence, be no more than it was in each before their junction, p. 613. How to diminiſh the danger of an overflow, occaſioned by the tides, wind, and roſſing in of the ſea at the mouths of rivers, p. 614. Advantages of ſluices for theſe purpoſes, p. 616. Which, however, are only uſeful when there is a ſufficient abundance of water in the rivers to fill the new beds and channels, ſo as to prevent the velocity of the currents therein from being notably diminiſhed from what they were before the diviſion, p. 618. Laws of the meeting of oppoſite currents, with the application of them to ſluices, p. 619. Sluices need be ſhut only a quarter of an hour before the flood has riſen to the level of water in the canal, p. 621. Experiments to determine the different velocities in different depths of water of the ſame floating body, moved uniformly by an equal force, p. 622. Dr. Franklin's experiments on this ſubject, repeated on a larger ſcale, with an account of the mechaniſm of the inſtruments uſed, p. 624. Table of the experiments, p. 629. And remarkable concluſion from the mean reſults of them, p. 630. On the quantity of declivity in rivers, p. 630. Principles to prove that the declivity and velocity of a river are leſs in proportion as the bed approaches nearer to being concentrical with the curve of the earth's ſurface, p. 633. The real quantity of declivity, in different rivers, determined as nearly as poſſible, ibid. Table of comparative proportions between the declivities and velocities in different kinds of rivers, p. 640. All the rivers in the world claſſed, p. 641. General view of the elevation of continents along the principal rivers of the known world, p. 654.

Rulandus. Cauk wetted with antimony has appearances of his falſe liver of antimony, p. 19.

Ruſſians, how they treat perſons affected by the fumes of burning charcoal, and ſimilar effluvia, p. 324. Ruſſian-houſes, how heated, p. 327.

S.

Salt. Sal ammoniac produced by clays diſtilled with ſea-ſalt, p. 21. Glaſs, in which there is much alkaline ſalt, leſs fit for electricity than others, p. 665.

Salt-water. Some relief obtained by it in the cure of a violent eruption on the face, neck, and breaſt, p. 54.

Sand. A progreſſive induration of maſſes of, at the bottom of the ocean, p. 37.

Selenite. Compariſon of the Montmartre plaſter ſtone, with ſeveral ſelenitical ſubſtances, p. 16.

Sheep. Dubious whether there are any Free-martins among, p. 287.

Shirl frequently, though erroneouſly, called baſaltes, p. 24. Contains nearly as much earth of allum as the Corniſh porcellane clay, ibid. Sal catharticus amarus obtained from pumice ſtone and ſhirl, ibid.

Silver. Experiments on the ore called argentum vitreum, p. 532. Concluſive experiment to ſhew that vitreous ſilver ore is a compound of ſilver and ſulphur, and that, when pure, it contains between ninety-two and ninety-three grains of ſilver in one hundred, p. 536.

Sitodium inciſum & Macrocarpon, or bread-fruit, deſcribed, p. 462. Liſt of authors who have given an account of it, p. 462. The plants and ſeeds of, how preſerved in a voyage from Ceylan to Europe, p. 463. Generic deſcription of it, p. 464. Specific deſcription of the Sitodium inciſum, p. 470. Obſervations on the Macrocarpon, p. 471. Various uſes of its parts, p. 471. Deſcription of the various diſhes made of theſe fruits by the Dutch, p. 476.

Slate, Iriſh, replete with allum, p. 25.

Spar. Change of cryſtal into ſelenitical, p. 12. The ſelenitical, or gypſeous ſpars, conſiſt of acid of vitriol, calcareous earth, and ſome clayey matter, p. 14. Some among theſe enumerated, which were not before taken for ſelenitical ſpars, ibid. No ſulphur in ſemitranſparent Auvergne ſpar, p. 15. Experiments on theſe compared with the common Montmartre ſtone, p. 16. Calcareous earth in the gypſum, ſhewn to contain more acid of vitriol than that in the ſelenitical ſpars, p. 17. Change of clay into Feld-ſpar, and different gradations of the change, p. 22. Allum made of feld-ſpar, p. 23. And of the Labradore ſtone, ibid. Account of ſubſtances which have a ſparry appearance, and how to diſtinguiſh them from ſpars, p. 27. Phoſphoric ſpar, cryſtallized, forms perfect cubes, p. 28. One exception only to this rule, ibid. Of a ſet of ſpars whoſe properties were not hitherto known, and experiments made on one of them, p. 29. Probable that the green and yellow glimmers from Johngeorgenſtadt may be of this kind, p. 30.

Stars. Account of an occultation of 1 ♋ cancri, by the moon, p. 165. Account of occultations of ſeveral, p. 178.

Stone. Conſolidated by cementing cryſtalline particles, p. 8. A formation of it to this very day, in certain places, more perfect than is imagined, p. 38. Portland-ſtone conſiderably hardened by being waſhed with water impregnated with ruſty iron, p. 44. Stones and marbles continually forming in the earth, and the conſolidation of them effected by means of water and vapour, and the induration of many of

those bodies effected by iron, p. 47. Ornaments in Portland, and other, might be preserved by being washed or brushed with water, in which there is a solution of iron, p. 48. Common sea-sand, with a small admixture of the solution of iron, may become a very useful stone, ibid.

Sugar-Cane. The two methods in use for cultivating it before Mr. Cazaud's, p. 210, 211, 212. Explanation of Mr. Cazaud's method, and objections to it, p. 213. Natural history of the cane, p. 218. History of its roots and productions under ground, p. 219. Should be put into the ground as soon as cut, p. 221. First productions, what they are, ibid. Roots of the first and second order described, p. 223. Roots of the third order, what they are, p. 224. Second productions described, ibid. Third productions described, p. 226. Proof of the necessity of planting in May, p. 227. Canes derive their nourishment from the three orders of roots for the whole of their duration, p. 228. Strong argument against the pretended antiquity of the stools, p. 231. And of the inutility of replanting when the stump is not raised above the ground, ibid. Rattoons derive their nourishment only from three orders of roots, p. 233. Advantage of cutting the canes in the ground, and mischief of the method of moulding up the stump, p. 235. Why rattoons are forwarder at the twelve months end than planted canes at the end of fifteen, ibid. And why those cut at the end of ten, eleven, or twelve months, are finer than those which have stood sixteen, ibid. Why fine rattoons are never got from grounds called exhausted, p. 236. Why those cut before the time, are those which stood best, p. 237. Difference in the time of jointing of the canes, and in the different numbers of the rows of their roots, what owing to, p. 238. Advantages of chusing the rainy season for planting, p. 239. History of the joints of the cane above ground, p. 241. Calculation that may be made of their number, ibid. Cane of the thickness of a pen, and only three inches long, with its two and twenty joints, distinct, p. 244. Fall of the leaf only criterion of the maturity of the joint to which it adhered, p. 245. Relative maturity of the cane, what it depends on, p. 246. Four thousand gallons of juice yielded equally by canes cut at ten and fifteen months end, p. 247. Deductions drawn from this fact, p. 248. Account of the cane in different soils, p. 250. The growth of a plant in America not to be estimated by what we see it in our hot-houses, p. 250. Forty-eight to fifty the greatest number of useful joints, p. 252. Effects of dunging the soil in which canes are planted, p. 261. Cane never grows to any purpose after the thirteenth month, p. 264. History of a singular revolution in the cane, and of the arrow which follows it, and constitutes the last stage of the cane's existence, p. 264. The quantity of the juice of the cane lessened by dry weather, ibid. Drying-up of the joints, p. 265. History of the cane according to the two different methods of cultivation, and in different years, the favourable, the dry, and the rainy, p. 270.

Sulphur. None in the semitransparent Auvergne spar, p. 15.

Tartar

T.

Tartar of vitriol. Mr. Beaumé miſtaken, in aſſerting that he obtained it from the blue Argilla, p. 20.

Teleſcope. Account of an iconantidiptic teleſcope, producing two images of the object, the one in a direct poſition, the other reverſed, p. 130. The direct paſſage of the center of a ſtar obſerved with this inſtrument, p. 131. The difficulty of illuminating the threads of the teleſcope obviated by this method, ibid. Why Caſſegrain's conſtruction of the reflecting, is preferable to either the Gregorian or Newtonian, p. 426.

Thermometer. Deſcription of one, for making experiments on the heat of boiling water, p. 360. Table for the uſe of artiſts who make them for ſuch purpoſes, p. 375. Regiſter of the, at Lyndon, for 1778, p. 545. Obſerved at Nain and Okak, p. 658.

Tin, ſuſceptible of a conſiderable degree of magnetiſm, p. 543.

Tin-ſpar of the Germans, experiments upon it, p. 26.

Tobacco-pipe, duly prepared, propoſed to be uſed by painters inſtead of white-lead, p. 20.

Tobacco-pipe clay. Allum may be produced from, p. 19.

U.

Ugar, what it is, and its effects, p. 327.

Vitriolic acid, more of it in gypſum than in ſelenitical ſpars, p. 17.

Ulna. Treatment of a diſeaſe of the lower part of the, where it joins the bones of the carpus, p. 10.

W.

Water. Firſt application of cold water to the human body produces heat, p. 330. The variation of the temperature of boiling, examined, p. 362. That the heat of boiling, is variable, known to Fahrenheit, ibid. Deciſive experiments by Le Monnier and Caſſini, to ſhew that this quantity is very variable, p. 363. Mr. De Luc's obſervations on this ſubject, repeated by Sir George Shuckburgh in a journey over the Alps, p. 364. And again in 1778, p. 365—367. True proportion of the preſſure of the atmoſphere, how obtained, p. 370. Sir George's obſervations compared with Mr. De Luc's, and with each other, p. 371—373. Water, which has ſtood ſome time expoſed to the

the ſun, yields a great quantity of dephlogiſticated air, p. 377. Experiments on air, extracted from various kinds of water, p. 432. Experiments on air from the water of a well, p. 433. And from the water of the Seine, ibid. And from that of the river Arqueil, p. 436. And from diſtilled water, p. 437. Water abſorbs a greater quantity of thoſe kinds of airs which contain a leſs quantity of phlogiſton, p. 439. Theſe experiments explain why ſome kinds of waters have a ſharper taſte than others, p. 440. And why they precipitate the lime in lime-water, ibid. Likewiſe why ſome waters diſſolve iron, and keep it diſſolved without any ſediment, ibid. Diſtilled water, deprived of air, imbibes an equal quantity of air of the ſame kind as that it had loſt, in leſs than fifty days, ibid. Pure water changes common air into dephlogiſticated, and is the beſt means of meliorating common air, ibid. Which may be one of the methods by which nature keeps the atmoſphere in a ſtate fit to ſupport animal life, p. 441. How to prevent the air being altered (in extracting the air from water by the action of fire) by the vapour of the water itſelf, p. 442. Common air, ſhaken in water, is ſenſibly increaſed in its bulk, p. 443. But dephlogiſticated air is diminiſhed, p. 444.

Weather. Obſervations on that of the year 1778, made at Lyndon, p. 548. At Briſtol, p. 553.

Weirs in rivers, the advantage of them, p. 594.

Weſley. Singular talents of two children of that name for muſic, p. 204.

Whirlpools. What they come from, p. 607.

White, Mr. *Charles.* A fact aſſerted by him, Vol. LIX. Art. 6th, of the Philoſophical Tranſactions, confirmed, and a proof given that a chirurgical improvement, propoſed by him in that article, may be extended to other parts of the body, p. 6.

Whiting. Experiment on dryed, p. 33.

Windward-Iſles. Obſervations on the climate of the, p. 216.

Winne, Mrs. Her early talents for muſic, p. 205.

Z.

Zinc. The flowers of, given without effect in a St. Vitus's dance, p. 2.

Zeolite. Experiments on the Norway, p. 27.

END OF VOL. LXIX.

The number of PLATES in this volume is IX.

ERRATA.

VOL. LXVIII. PART II.

Page Line

1040. 12. *for* conducting, *read* non-conducting.

VOL. LXIX. PART I.

18. 19. *for* wetted with antimony, *read* heated with antimony.
16. 15. *for* an ounce and a half, *read* a drachm and a half.
24. 5. *for* 5 ounces 36 grains, *read* 5 drachms 36 grains.
27. 4. *for* Zoolite, *read* Zeolite.
30. 19. *for* tin, *read* fire.
122. 10. *for* percurfio, *read* percuffio.
—— 14. Idem.
—— 19. *for* percurfionibus, *read* percuffionibus.
124. 15. *for* percurfiones, *read* percuffiones.
126. 12. *for* percurfione, *read* percuffione.
—— 20. *for* percurfionis, *read* percuffionis.
151. 14. *for* it may obſerved, *read* it may be obſerved.
182. 2. *for* Stephens, *read* Stevens.
243. laſt line of the Engliſh, *for* pea, *read* pen.

VOL. LXIX. PART II.

364. column the 4th, *for* Mr. de Luc's thermometer, *read* Mr. de Luc's rule.
373. column the 6th, line the laſt, *for* 96, *read* 98.
Ibid. column the 7th, line the laſt, for 0, *read* +0,2.
380. 15. *for* open on both ſides, *read* open at both ends.
540. *for* working-needle, *read* ſewing-needle, *paſſim*.

INDEX.

684. *for* Eric Proſperin, *read* Proſperin Eric.
685. 22. *for* piſtol to make it, *read* piſtol with it.
690. 16. *for* one for making, *read* one for firing.
692. 30. *for* wetted, *read* melted.